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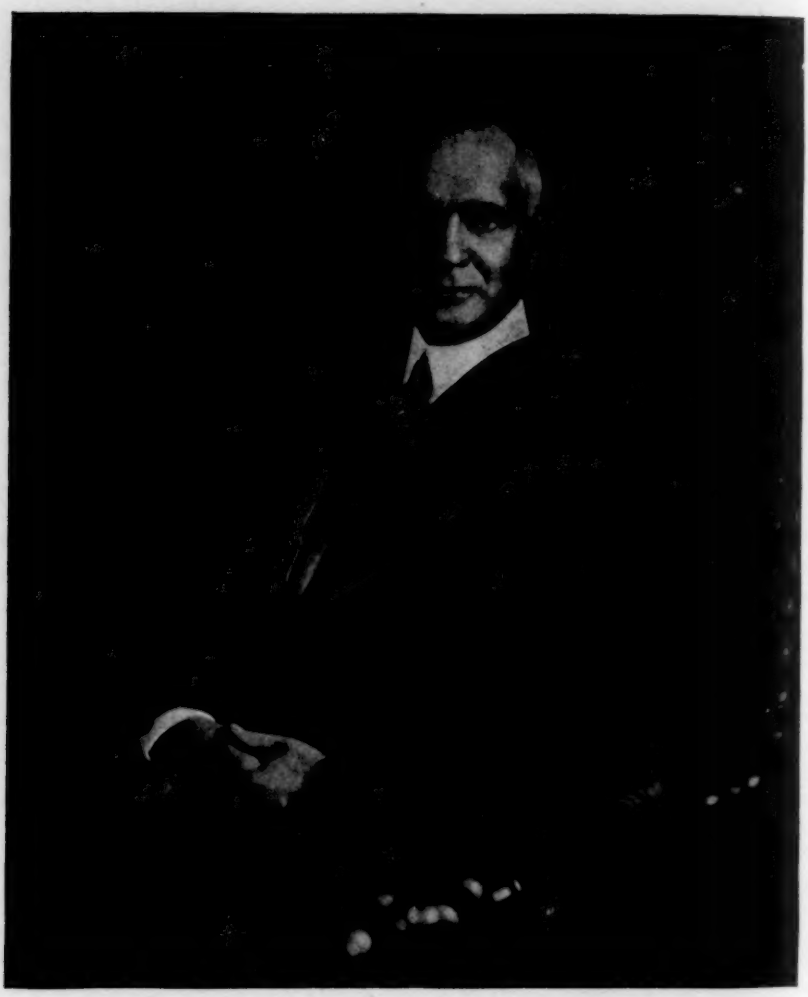

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Vol. 205 MARCH, 1928 No. 3

LIGHTING IN INDUSTRY.*

BY

DUGALD C. JACKSON, C.E.,

of the Massachusetts Institute of Technology; Chairman of Committee on Relation of Quality and Quantity of Illumination to Efficiency in Industry.

IT is usual to allow one's meditations to rest on steam transportation by land and sea, and on electric communication of intelligence, equally by land and sea, as twin supporting pillars in the industrial civilization of our present era. That is quite right. Each is the outcome of experiments and discoveries beginning in the latter part of the eighteenth century which laid bare the germ-like buds of the principles which underlie our practice of today. Further discoveries and inventions made during the first part of the nineteenth century expanded the buds to full flower. Additional blossoms have been and still are being added continually.

These twin agencies, transportation and intercommunication, however, are more than supporting pillars of an industrial civilization. They have entered into the intelligence of nations and expanded their souls. They have changed the aspects of life for common people in most of the world. If they have not yet made the world of one

* Presented at a meeting held Thursday, December 8, 1927.

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heart, they have wrought the approximate miracle of making all harvests throughout the civilized world available to serve hunger wherever it may be. They have killed the old specter, famine, in all civilized parts of the world.

Such thoughts are characteristic in this age. We rejoice in the advantages derived from these servants of civilization. We strive to expand their reach. We improve our practice in their use and we add to our methods of applying power in both transportation and communication. Why do we not give equal reflection to methods of lighting? The earliest use of artificially produced light by means of some sort of a torch seems to be lost in the obscurity of prehistoric antiquity, like the earliest use of the wheel for purposes of transportation. We can only guess the manner of discovery and application of each by some individual, who was less muscle-ambitious and more brain-ambitious than general humanity in the crude prehistoric days. A slow and gradual improvement of lighting devices occurred while history and civilization gradually unfolded, but (as in transportation and electric communication) the discoveries and inventions in the latter part of the eighteenth century and the first half of the nineteenth were required to put artificial illumination in the garments of a hand-maiden of civilization.

Much of our daily illumination comes to us free, like air and water. We therefore fail to consider the benefits conferred by artificial illumination of high quality which lengthens the hours for creative work and recreative play. When these advantages are analyzed, it is plain that, in importance to our industrial age, artificial illumination now stands a peer of transportation and quick intercommunication. Do we sufficiently utilize the qualities which it affords? The research which I will describe is an attempt to gain a partial answer to that question, confined to illumination in the industries.

Finding itself with a gift of funds for the purpose, the National Research Council, through its Division of Engineering and Industrial Research, formed a committee with Mr. Thomas A. Edison as Honorary Chairman and an eminent

membership of industrialists, publicists and scientists, and requested the Massachusetts Institute of Technology to provide headquarters and men for carrying on the research. A number of industries offered privileges and aid for carrying on tests and investigations in their manufacturing and clerical departments; and certain colleges were hospitable in the provision of laboratory facilities. One of the most ardent progenitors of the work was the late eminent physicist Dr. Ernest Fox Nichols, whose lamented death was a loss not only to this investigation but to all physics.

SCOPE OF THE RESEARCH PROBLEM.

The scope of the problem under investigation was defined by the National Research Council as the "Relation of Quality and Quantity of Illumination to Efficiency in Industry"—an important, but very large and difficult subject for a research. It involves rate and quality of industrial production and equally involves the obscure subject of industrial fatigue. Quality of illumination was interpreted to comprise distribution as well as color of the light. Many scientific principles of somewhat contradictory and numerous confusing characteristics are involved. A program was laid out for investigation covering a period of two years, which was anticipated to lead to some definitive results of value, and indicate where value might be found in further research. In realization, the research program took three years and the report of the total project is in course of preparation.

Some factory lighting investigation within the scope of the subject had gone before; but much of the work was of the nature of imperfectly controlled observations, and conclusions drawn from these observations were open to the criticism of being warped by conditions that were not taken into due account. It was needful to apply more searching methods of research to the problem in order that data might be secured from which reliable conclusions could be drawn. The program therefore had to be basic, and began with the formulation and experimental development of an adequate technique, which was no small undertaking in itself. One of

the previous investigations was that of Dr. James E. Ives of the United States Public Health Service (now a member of the research committee) who investigated the influence of altered illumination on letter sorting by postoffice employes; which gave a number of clear-cut results.

FOUR BRANCHES OF THE RESEARCH.

The problem involves three branches of science: Physics (of light), Psychology and Physiology. The physics of light has many facets, like transmission, reflection, interference, intensity, color, overlap of colors and the like. The worker's psychology, the physiology of illumination, and the influence of illumination on fatigue are seemingly infinitely complex. The research itself seemed conveniently divisible into four coördinated branches, each including within its borders all three of the branches of science before named, since they are indivisible in their bearing on factory relations, and the relations are not fully divisible even in the most carefully guarded laboratory researches into the effect of illumination on the mental and physical performance of the human kind.

These four branches of the research are:

First. A study and analysis of the scientific and industrial literature to learn definitively what had gone before and what therein could be used as a sound basis from which to start the new research. Dr. Leonard T. Troland of the research committee undertook a considerable responsibility for collating and analyzing scientific literature within the field of the research, especially as far as it touched the aspects of psychology, and his written report on Analysis of the Literature Concerning the Dependency of Visual Functions upon Illumination Intensity has been of primary value in showing within what range there exists experimental knowledge which can be relied on, and also in showing the tenuous character of most of the joint psychology-physics data regarding illumination. Another analysis by Dr. Troland which was put in writing under the title "The Problem of Deducing the Proper Illumination Intensity for Any Given Industrial Operation" had value in association with our final critical examination

of the validities and faults of our experimental procedure and technique.

Second. Some experimental work by psychologists required to fill up certain lacunæ in information regarding sensitivity, discrimination, acuity and fatigue that seemed important in connection with our procedure. This mostly fell on Doctors Ferree and Rand of the Department of Experimental Psychology of Bryn Mawr College, where the college laboratory was well adapted for the purpose and the experimenters were well experienced in the field. Certain of this work has been published by Doctors Ferree and Rand through the Illuminating Engineering Society in 1925, 1926 and 1927.

Third. Experimental work of a laboratory nature or under controls corresponding to those of a laboratory made either in the laboratory or in the industries themselves for the purpose of securing an understanding of the relations of scientific investigations in the subject (which have been made in laboratories) with the conditions under which their conclusions may be applied when they are carried over into industry. Work of this character was carried on for the research committee by Dr. Irving C. Whittemore of the Department of Psychology of Boston University, in a room specially fitted up with illumination facilities by the Edison Electric Illuminating Company of Boston, by Dr. Johnson O'Connor at the West Lynn Works of the General Electric Company, and in electrical engineering laboratories of the Massachusetts Institute of Technology, besides work by Doctors Ferree and Rand at Bryn Mawr College.

One of the heavy difficulties in making out a procedure for definite scientific research in a situation like the one here concerned is that psychology investigation in university laboratories generally relies on one or two individuals as subjects. This may not produce results appropriate to unqualifiedly apply in respect to factory workers with their multitude of mental and physical influences widely differing from those that may bear on the university experimenter.

Another is the mental reluctance of some psychologists to acknowledge the difference in kind between a usual university research laboratory and a production department of a great industry. This distinction between the two cannot be broken down on one side without destroying the singleness of purpose and exactness of aim which gives to laboratory investigations their merited repute for refinement and accuracy, or on the other side without destroying productive methods of the factory. The psychologists who fail to recognize this difference in kind would attempt to finally solve all industrial problems in the laboratory, which we know cannot be done; or they would endeavor to clap laboratory restrictions on the factory, which we know would interfere seriously with production, as well as arousing discontent of workers because of the limitations on their personal liberty and individual wills. Happily, the professional psychologists who are members of the research committee, Dr. Knight Dunlap of Johns Hopkins University, Dr. L. T. Troland of Harvard University, Dr. C. E. Ferree of Bryn Mawr College, and Dr. C. E. Seashore of State University of Iowa, freely visualize the conditions, and their aid and advice in the research was accordingly large.

Fourth. The fourth coördinated branch of the research consists of studies of the factory workers, measurements of factory production under different conditions of illumination, surveys of eye strain and general fatigue, and the adaptation of conclusions and inferences secured in the other branches of the investigation to application in the procedure, observations and conclusions of this branch, along with suitable verification of the soundness of the conclusions regarding "workers' efficiency." This, of course, leads to the main objective and the three other branches are only collateral aids to following out this branch with scientific soundness until conclusions of some definitive nature may be relied on. The phrase "workers' efficiency" is here used in an inclusive way and therefore involves personal effects on the worker like health and fatigue in addition to production output.

In pursuance of this branch of the research, studies, tests and experimental investigations were carried out in production

FIG. 1.

Weaving loom under old illumination. John Sidebotham textile mill, Philadelphia. Observe heavy shadows at the heddles.

departments of several important industries, in some instances extreme care being taken to avoid modification of the usual conditions of production except the illumination effects, and

in others intentionally modifying other conditions in a definite way for the purpose of securing a more effective experimental "control." The principal observations and investigations of this nature were carried on at the Hawthorne Works of the Western Electric Company, the Framingham Works of the Dennison Manufacturing Co., the Bridgeport Works of the General Electric Co., the Plainville Works of the Whiting and Davis Co., and the Philadelphia Works of John Sidebotham, Inc. I will not here go into the details of these several projects, each of which proved fruitful to the research; and certain of them, that were carried on during successive years, were far more fruitful than original anticipations promised. Lesser studies have been made in other industries, but all typical of industries aggregating great numbers of employes. Full details will be published in the Committee's report. In the meantime those who are interested in a technique for such tests can refer to a publication in Transactions of Illuminating Engineering Society entitled "Technique of Economic Studies of Lighting in Industry," by Professor J. W. Barker, who has been Principal Research Assistant in the work.

In planning shop studies, the question of taking the workers into confidence by informing them that tests were to be made and how they would be executed became one of rather serious import. It was desirable that the workers should not be mentally disturbed by unusual observations being systematically instituted, or by changes in working conditions through changed illumination. It was finally concluded to be best to inform the workers, who would come into a test, about it in advance, making them, to some extent, interested observers. It usually is impracticable to make an industrial research such as this without the involved workers knowing that something unusual is going on, and ignorance of the facts might stir up antagonisms sufficient to destroy the validity of any observed data, or even to make continuance of the observations impossible. The Committee is indebted to such fellow members as James P. Noonan, Vice-President

of the American Federation of Labor, and C. G. Stoll, Vice-President of the Western Electric Co., for excellent counsel.

FIG. 2.

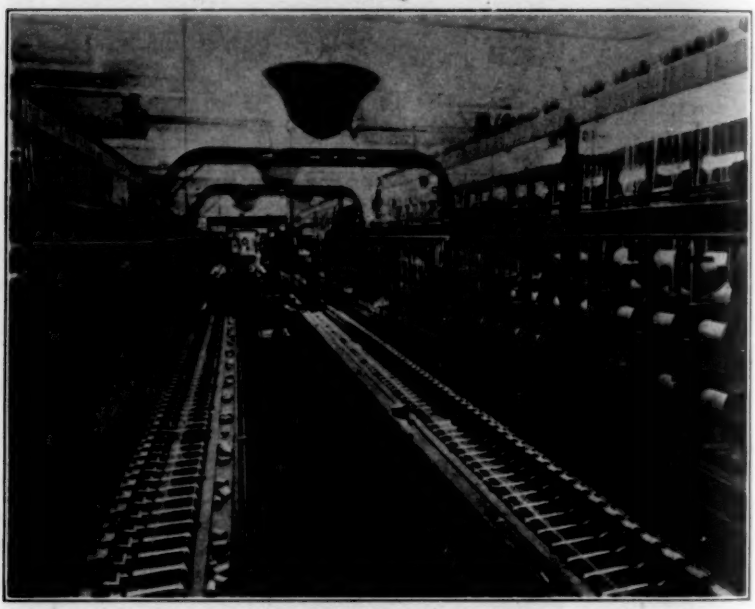
Same loom as in Fig. 1 when subjected to improved illumination. Observe clearness of threads and absence of shadows.

The psychologists of the Committee also had a hand in the solution.

SOME MAJOR FACTORS OF THE RESEARCH.

A feature in any investigation of illumination is the permanent effect, whether beneficial or deleterious, on the eyes of those immersed in, or working in the margin of, the illumination. There has been much idle gossip in popular circles and some, I confess, in scientific circles, about the unwholesome effect on the eyes, of this or that, in the way of lighting. Exact knowledge, given general circulation, is de-

FIG. 3.



General view of long loom bay after introduction of improved illumination. John Sidebotham textile mill, Philadelphia.

sirable. This feature stands out notably as worth following to a conclusion, because the considerable number and diversity of workers involved in the observations of this research indicate that it might be practicable to secure a wide range of eye data from which statistical conclusions of some importance would be derived.

Dr. George S. Crampton of the University of Pennsylvania took upon his shoulders this feature, and numerous data have

been gathered. As far as the matter goes, I think that we confirm the well-known benefits to the efficiency and physical well-being of workers which arise from using glasses to correct lenticular defects which exist in the eyes of any individual; and also the well-known fact that the perfect eye under the influence of varieties of illumination does not alter in a few months, if at all, sufficiently for changes to be detected by the usual tests of the ophthalmologists. Whether Dr. Crampton ultimately may derive more constructive results from the eye data, time only can answer.

A committee on the Atmosphere and Man (which is a joint committee of the Division of Biology and Agriculture and the Division of Medical Sciences, both of the National Research Council) of which Dr. Ellsworth Huntington of Yale University is chairman, is carrying on an investigation relating to the effects of atmospheric conditions on industrial employes, and we have hoped that some contribution to the fatigue side of our research may come from them, in addition to what we may extract from our own data. Fatigue of industrial workers has been followed quite continuously by official committees of Great Britain since the early part of the war and their investigations are beginning to bear fruit in the way of apparently verified results within a narrow range. Where the data and conclusions of the British Committees, and especially of the committees of the government Department of Scientific and Industrial Research or of the Medical Research Council, relate to the incidence of illumination conditions on output or fatigue, or on the relations of illumination or fatigue to accident rates, we have found their experiences characteristically similar to our own. I will speak farther of certain of their results later on.

The relation of accidents to conditions of illumination has been and still is one of our most disturbing problems. In other directions, we seem to be able to secure data of reliable nature. Preconceived notions of our observers have often been shattered and reasons for observed facts often remain obscure as yet, but the data themselves can be secured by

putting research assistants at work under properly controlled conditions in the factories. The situation respecting accidents, however, is different. Reasoning *a priori*, one readily persuades oneself that industrial accidents are intimately related to conditions of illumination, and much has been written regarding the relation, apparently on the assumption that such data are extant. A careful critique, however, leads to the opinion that data adequate to prove the relationship have not been published, and that the extent of the relationship is yet to be proved. Possibly the most important aspect of poor illumination versus accidents may be traced through the influences of different conditions of illumination on mental alertness and on fatigue, but the quantitative incidence which either alertness or fatigue may have on the accident rate has not yet been shown. Indeed the stimulus of suitable illumination on mental activity, alertness and sense of well-being (cheerfulness), a very important point, has not yet been suitably investigated by professional psychologists; nor has the reaction of that stimulus in preventing or retarding the phenomena of fatigue. We are still diligently seeking information, although our active research is closed for the present for the purpose of preparing our Committee report. After comprehensive data have been gathered in a scientific, statistical manner by some agency it will be necessary to eliminate the influence of special susceptibility of a few employes (a phenomenon discovered by the British Committee) and other influences disturbing to accuracy of conclusion, before the data can be used effectually.

The recently formed French Commission de l'Eclairage des Usines et des Ecoles, of which Maurice Leblanc is President, handsomely defined the object and qualities of good illumination in a prospectus of the objects of the committee. The original is to be found in Vol. 5 (year 1925), page 695, of the *Bulletin de la Societé Francaise des Electriciens*. It may be freely translated to say that the lighting in all parts of a factory should be of such a character that workers at any location may execute their work under conditions encouraging

rapidity and excellence because of the wise consideration of optic hygiene.

This quotation, the reports of the British Committees already referred to, various studies by semi-official German committees, and the program carried on by our committee show in common an international recognition of the importance of proper lighting in industry, and a sentiment that the industries as a class are not characterized by proper lighting. We doubtless all substantially concur with the French definition of what constitutes proper factory lighting, viewing it as a philosophical statement of aim. Transforming the definition into a workable basis of application is the end to be accomplished. This requires a complex formulation of relationships in physics, physiology and psychology. Threshold relations of the eye in absolute sensitivity, discrimination of brightness, effect of color, effect of reflection coefficients, visual acuity, brightness contrasts, color contrasts, mixture of colors, flicker, dazzle or glare, contrast between illumination on an object and intensity of surrounding illumination; the relations of all these to eye reaction as a function of intensity of illumination; eye adaptation to brilliance, color, distance, etc.; the conversion of the eye reactions with each of these conditions into a phenomenon called "speed of vision" as a function of intensity of illumination; besides the purely physiological-psychological phenomena of relations of quality and quantity of illumination to cheerfulness and to fatigue: these are scientific elements which must be recognized as variables affecting the research. Social, physiological and psychological elements also affecting the research are home life conditions, health, wage methods, supervisory methods in the shop, shop equipment, individual temperament of workers, and the like. All introduce variables into the data which may mask influences due to illumination *per se*. This array rules out any successful establishment of a formula, except it be for very simple conditions, and such conditions are not characteristic of the problems of lighting in the industries. Actual measurements under factory conditions of the response

of "workers' efficiency" to varied illumination have to be resorted to, but laboratory researches may be associatively used.

TESTS IN THE FACTORY PROVED TO BE ESSENTIAL.

Dr. Troland summarized the situation thus in his report on deducing the proper illumination intensity for any industrial operation: "Many of the psycho-physical principles which are involved are quantitatively and reliably established. It is probable that any concrete industrial situation can actually be analyzed so as to become amenable to these principles. Experience shows that production tests are subject to powerful, irrelevant disturbances and that their results are difficult of interpretation and subject to the influence of prejudice." Therefore (at the instance of the committee) he set about investigating the possibility of a guiding definite formula that would apply to a typical industrial operation for predetermining the "proper illumination intensity." You will observe that his statement harks back to psycho-physical principles which have been established and therefore assumes particular eye characteristics, namely, the eye characteristics of laboratory workers. We may pass this over, assuming for the momentary purpose that these eye characteristics do not materially differ from those of factory workers in general when any serious existing optical defects of the latter have been corrected by glasses. Moreover, his assigned problem, the "proper illumination intensity," deals with only a part of the total problem of illumination, since it refers to intensity only, while distribution and color are influential factors in the utility of illumination. We may pass this over also, since research must proceed in the order of component analysis antecedent to a comprehensive synthesis, and a reasonably exact expression established for deducing the proper value in a fixed industrial situation of even one factor of intensity (such as brightness discrimination) would be applaudable.

This being the scene, let us see what solution it leads us to. I will again quote Dr. Troland. "At the outset of the

present study, the writer was of the opinion that it should be possible to develop from existing data a quantitative method for reaching the desired conclusions. However, as the work has proceeded, it has become progressively clear that the data in question [of psycho-physical science] are sadly inadequate. Even in the most investigated fields, such as that of brightness sensation, the system of laws is fatally incomplete and the studies of different investigators do not check and dovetail with one another in the required manner. . . . The first trouble comes in our inability to determine all the facts, and reduction to practice is necessarily postponed. . . . Considering the problem, first, from the general psychological standpoint, we note that response—including muscular reactions—rests upon perception and that perception in turn depends upon various processes of discrimination. For the visual case before us we recognize ten different forms of discrimination, each of which is a function of numerous stimulus and other conditions. These conditions determine whether or not discrimination can occur, and their minimal values for such occurrence underlie the respective discrimination thresholds. We assume that if the threshold is passed, perception and response will follow, with relatively little loss of efficiency as compared with the results of supraliminal values. However, the bearing of variations of stimulus conditions above the threshold upon speed and accuracy is considered. . . . In order to carry out the above general procedure we require an exact formulation of the functions which relate the ten visual discrimination processes to their conditions or determinants. . . . Although some progress is made in this attempt, the final conclusion is that the data are not sufficiently complete and consistent to permit the formulation of an exact system of deduction even for the case of brightness discrimination. As indicated by a tabulated analysis, the situation is even less favorable with respect to the other nine discrimination processes."

And this again proves that we are thrown back upon the complex and difficult task of investigation in the factories,

until psycho-physical data relating to visual phenomena which may be investigated in the laboratory are very much more fully worked out. Certain empirical rules that are based on laboratory science may be used as tentative guides in situations to which they appear applicable: for example, the Rule of Thirds (*loi du tiers*) ascribed to Professor Broca as a consequence of his laboratory investigations of visual acuity. But the basis does not exist for generalizing even in these. The paucity of actual knowledge regarding the benefits and characteristics of proper lighting have prevented their full utilization; suitable fixtures have not always been available; and expenditure for a tenuous advantage has seemed inappropriate. Some installations of high intensity illumination have been a dis-service instead of a service, and have proved expensive mistakes, because of failure to recognize that quality (usually in terms of reasonable sources and suitable distribution) must be established before moderate and higher intensities can be endured. Nature in this respect is still ahead of man.

ENUMERATION OF A FEW OF THE RESEARCH RESULTS.

We may now turn attention to the research results, naming a few of them briefly. I will ask you to kindly consider all expressions regarding the research results as personal opinions and tentative, since the final data have not yet been submitted to the committee at large and I wish to avoid being charged with pre-judging.

(1) Old style factory buildings with inadequate window lighting may be put on an equality with modern window spacing by using properly arranged artificial lighting, and therefore capital in existing plant may be conserved with economy.

(2) In such situations, as well as others where artificial lighting must be relied on for considerable periods of operation, conditions of general manufacturing indicate benefit from illumination of good distribution and of an intensity of from 5 to 10 foot-candles in the working plane. Dr. Ives' postoffice tests show that not less than 8 foot-candles are desirable for

usual letter sorting, which is an independent confirmation in an associated field. Some kinds of work require more intense illumination to secure best "workers' efficiency." For example, in the Sidebotham textile mill an illumination which gives about 12 foot-candles at the heddles of the looms seems most satisfactory, according to our research. The British Industrial Fatigue Research Board and Illumination Research Committee jointly have a similar result, where they show that workers' efficiency went up in a test of typesetting by hand, in the printer's trade, until the effective illumination exceeded about 20 foot-candles, and that the efficiency then rivalled that secured with day lighting.

(3) Per contra, many situations, even among those involving processes of rather fine work, do not need so much light for the safe and rapid performance of the processes, and benefit from the given figures over lower levels of illumination then seems to depend on other factors, such as the influence of excellence of illumination on morale.

(4) Factories on an eight-hour regime in modern buildings with large window exposure may secure but little direct benefit from improved artificial lighting because the use of artificial lighting is required such a small part of operating hours; but they may receive an indirect benefit from improved morale during the months of dark afternoons. When on double shift, the efficiency of the overtime work may be considerably influenced by the quality and quantity of lighting.

(5) As far as our Committee's data have been analyzed no conclusive difference on "workers' efficiency" is indicated between day-light, artificial light, and a mixture of the two, provided the intensity and distribution of the illumination are the same for each. The British Committee on Lighting of Factories and Workshops, seems to have arrived at a like result. After investigating conditions in several industries and finding no definite distinctions, and making some tentative experimental comparisons in a special laboratory, they stated that "no conclusive results indicating any special effects of mixed lighting were obtained" and dropped the investigation.

(6) As far as our analysis goes it appears that high intensity illumination of good quality (for example, well guarded against glare which dazzles) has no injurious effect on eyes, but that defective eyes suffer a greater inconvenience than perfect eyes from low levels of illumination. Correct glasses worn by the owners of the defective eyes are a cure for this difference, provided the defects are optical. These facts accord with the usual views of oculists, but have been disputed by many industrial employes.

(7) Measurements of fatigue in some absolute units seem yet to be discovered, and it therefore is proving difficult to associate illumination and fatigue in functional relation. There appears to be a relation, and something more tangible possibly may be disclosed by further statistical study.

(8) The retardation of fatigue also seems to be affected by morale, and therefore to be influenced by the stimulating effect of cheerful lighting.

(9) No definite relation between illumination and the industrial accident rate has been demonstrated either in this country or abroad, as far as previously published data or our data show. Common sense points out that accidents in the general walks of life occur in dim light which do not occur in properly distributed bright light. The fault in applying such reasoning to industrial conditions is that here we wish to learn whether low, medium, high-medium and high intensities of well-distributed illumination are superior, one over the other, in producing immunity from accidents. The subject is fraught with experimental difficulties, as we cannot deliberately submit human subjects to dangerous conditions for statistical purposes, and reliance must be had on improved statistical reports from the industries plus theoretical consideration.

Now, a further general word respecting the research. All the expenditure of effort and money required for this research would, in my judgment, be recompensed if no other outcome had been secured than the demonstration that older type factory buildings with inadequate window space may be restored by the suitable installations of artificial lighting to a compara-

tive level of usefulness approaching modern buildings. Other useful results, however, are rather definitely established, some of them being indicated above.

Certain additional investigations which are collateral to the main thread would add much to the collective value if money were available to carry them on. For example, Dr. Dunlap has devised possible plans for learning the degree of mental stimulation that is associated with high intensity illumination, and the consequences on mental alertness. Further information is still needed on fatigue effects and accident rate in function to illumination. Value to the industries cannot fail to come out of such further studies if well devised, and I express the hope that the Division of Engineering and Industrial Research of the National Research Council may be provided with funds so as to carry these three studies through. But, after all, the present big need in this field is encouragement and support for psycho-physical research in the field until principles are discovered, verified and enlightened sufficiently. Then the art of illumination may be established on a level of rational scientific applications perhaps equalling the level now enjoyed by the of art communication or the art of transportation.

Chemical Warfare against the Boll Weevil. H. W. WALKER (*Chemical Warfare*, 1927, *13*, 231–237) describes the work of the Chemical Warfare Service, United States Army on experiments in exterminating the cotton boll weevil. An extensive study was made, using over 1000 poisons and poisonous mixtures. Two materials were developed at Edgewood Arsenal and found to be commercially possible boll weevil poisons, a special calcium arsenate and a special sodium fluosilicate. The special calcium arsenate contains 20 per cent. arsenic anhydride, and is made by heating calcium carbonate (precipitated chalk) and arsenious oxide (white arsenic) in the presence of an excess of air at a temperature of 650° C. The special sodium fluosilicate is a byproduct in the manufacture of acid phosphate, and contains approximately 80 per cent. sodium fluosilicate and 20 per cent. colloidal silica. Both insecticides have a toxicity for the cotton boll weevil at least equal to that of commercial calcium arsenate, and do not burn or damage the cotton plant. Barium fluosilicate is an excellent insecticide for this purpose, but does not possess sufficiently increased toxicity to replace the other less expensive compounds. The special sodium fluosilicate acts on the weevil more rapidly than does the special calcium arsenate, but, at the present stage of development of the process of manufacture, does not stick to the cotton plant so readily. These insecticides have been used as dusts, but possibly may be applied as syrups. J. S. H.

Manganese in an Ancient Lake.—(Department of the Interior. Memorandum for the Press.) Manganese is a constituent of most rocks and is readily dissolved in ground water and reprecipitated. In the belt of lakes that lie along the south edge of the area covered by glaciers in the northern United States during the continental glaciation manganese and iron oxides have been found, here and there, in quantities that warranted mining. Most of these deposits have not yet been covered by sediment, but there was recently discovered in southern Idaho a minable bed of manganese oxides that had been deposited in a lake of the glacial period and later covered by 100 feet of silt. Up to May 1, 1927, it had yielded 1,200 tons of high-grade manganese ore. Proof of its ancient origin was shown by the presence of several teeth of an extinct mammoth under the bed. Bulletin 795H of the Geological Survey, recently published, will give further details.

THE THEORY OF THE ELECTRICAL BREAKDOWN OF GASES AT ATMOSPHERIC PRESSURES.

BY

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THE general theory of the electrical spark, as advanced by Townsend [1] some twenty years ago, has been and is today accepted as essentially describing the phenomena. In 1923 Holst and Oosterhuis [2] and more recently Taylor [3] have assailed certain assumptions made by Townsend on the basis of measurements made at low pressures in the inert gases with different electrode materials. Their criticisms, while cogent, are more directed at developing their own theories, so that they miss some of what appear to the writer to be the essential significance of the phenomena. It is the purpose of this article to point out these facts with the hope that more work will be done on these features in laboratories equipped for such work.

Townsend's theory in brief is the following. His study of the increase of the negative ionic saturation current at high values of the field strength X as a function of d, the distance between his plane parallel electrodes, led him to assume that the electrons, or negative ions as they were then assumed to be, produced α new ions per cm. path in the gas. His measurements of the currents as a function of plate distance led him to values of α and he found that one could express the values of α in terms of a relation $\alpha/p = F(X/p)$, where X is the field strength in volts per cm. and p the pressure in mm. of mercury. A kinetic theory of α/p as a $F(X/p)$ was developed by Townsend on an erroneous basis * which he has since recognized; [4] it was enlarged by Bergen Davis.[5] Following the discovery of elastic electronic impacts the theory was worked out for helium by Compton and by Compton and Benade,[6] who obtained excellent numerical

* He assumed that negative ions caused the phenomenon and that they suffered inelastic impacts with the molecules.

agreement. This theory had a weakness in that it used an erroneous equation for the probability of ionization by electrons on impact and neglected resonance impacts. With our present day knowledge, however, the separate phenomena involved are fairly well known, but the deduction of a proper theory presents such mathematical difficulties as to make it unlikely that a satisfactory general equation applicable to all gases can be made. At any rate the equations used make it plain that the interpretation of α/p as a $F(X/p)$ given by Townsend can be ascribed to electrons striking gas molecules, and that at values of X/p for sparking at atmospheric pressures (e.g. $X/p = 40$ for air) the values of α are probably great enough to account for the observations.

As Townsend in his experiments still further increased his plate distance d, he found that the equation containing α alone did not suffice. Analysis of the phenomena showed him that an equation satisfying the experiments within the limits of error could be derived by assuming that positive ions produce ions at the rate of β *pairs of ions per cm. of gas, by impact with the gas molecules.* This equation has the form

$$\frac{n}{n_0} = \frac{(\alpha - \beta)e^{(\alpha-\beta)d}}{\alpha - \beta e^{(\alpha-\beta)d}} \tag{1}$$

and is his famous equation which leads to the theory of sparking. Here n is the number of ions observed for a plate distance d and n_0 is the initial number of ions generated through external sources by ultraviolet light or radioactive ionization. The condition for a spark is then one in which the current becomes self-maintaining without the introduction of n_0 outside ions. That is, a spark occurs when, for practical purposes, n_0 approaches 0 and yet gives a current in the gas. The question raised about the precise mathematical interpretation of this has been adequately treated by Seeliger.[7]

In order that n/n_0 shall become very large, $\alpha - \beta e^{(\alpha-\beta)d}$ must approach 0. The criterion for sparking then is that

$$\alpha = \beta e^{(\alpha-\beta)d}, \tag{2}$$

which can be modified for the more general case of non-uniform fields as desired.[8] The physical meaning of the equation (2) is that β must reach such a value that enough electrons are liberated by positive ions from the neighborhood of the cathode to make the discharge self-maintaining. If d be kept constant, then a certain relation between α and β, which is determined by the different rates of change of α/p and β/p as functions of X/p, must exist. The study of this relation leads one to a study of the value and nature of β.

Townsend determined β from equation (1) and again found that experimentally β/p could be expressed by the relation $\beta/p = \phi(X/p)$. The theoretical treatment of this variation is somewhat simpler than is the case for electrons and one can without serious error apply the original equation of Townsend for α/p as a $F(X/p)$ to that for positive ions with appropriate modifications, as will be done later. The use of β for positive ions at low velocities may be questioned by some on the basis of the controversy concerning the ability of positive ions with but a few times the ionizing energy to ionize on impact with molecules.[11, 12] In a recent article the writer [12] believes he has clarified this question and the objection is not serious.

Townsend and his pupils [1] measured β for large values of X/p (from 100–1000) * and found that the equation held satisfactorily. Townsend used the values for α and β obtained to calculate the minimum sparking distance δ for low pressures with considerable success.[13] He also claimed that the mechanism assumed in his use of the constant β was supported by the independence of the sparking potential of electrode material observed in air and other gases at higher pressures and in all but inert gases at low pressures.[9]

It is here that Holst and Oosterhuis [2] and Taylor [3] launched their criticisms, for they found that in *inert gases carefully cleaned metal cathode surfaces influenced the minimum sparking*

* Except in He, where values of X/p down to 30 were studied, and in H_2, where some of the values worked with were near 50, most of them, however, being greater than 100.

potential at low pressures by important amounts. This would not hold if β were a function of the gas alone, as Townsend assumed. It indicates an action at the surface of the cathode. Townsend [14] himself, as well as J. J. Thomson,[15] had postulated the possibility that the electrons needed to maintain the discharge might come from the cathode; and Townsend had even derived the equation for this. If we assume that for every positive ion generated a fraction γ of electrons is liberated from the cathode either by *ultraviolet light from the positive ions* or *impact of positive ions with the cathode*, the general equation becomes

$$\frac{n}{n_0} = \frac{(\alpha - \beta)e^{(\alpha-\beta)d}}{\alpha(1 + \gamma) - (\alpha\gamma + \beta)e^{(\alpha-\beta)d}}, \tag{3}$$

which becomes for $\beta = 0$

$$\frac{n}{n_0} = \frac{e^{\alpha d}}{(1 + \gamma) - \gamma e^{\alpha d}} \tag{4}$$

and if we put $\beta' = \gamma\alpha/(1 + \gamma)$, one has

$$\frac{n}{n_0} = \frac{(\alpha - \beta')e^{\alpha d}}{\alpha - \beta' e^{\alpha d}}, \tag{5}$$

the equation given by Townsend. In view of the small value of β relative to α, Townsend [17] says that it gives as close an experimental agreement as did his original equation (1) involving β and no γ. He however points out that in the discharge from positive points [18] and wires the mechanism can only be explained on the basis of β, a fact which cannot be denied.* In this case the fields existing are much higher than for sparks between plane parallel electrodes. The potential for the discharge from positive points is in reality higher than that from negative points by some 25 per cent. to 30 per cent., so that it may be suspected that the negative point brings both types of action into play (equation (3)),

* Again, whether the positive ions having the energy actually do the ionizing or whether by impact they impart it to neutral atoms, which can ionize more readily,[12] is immaterial; the positive ions are the active agents here directly or indirectly.

while the mechanism with the positive point depends solely on equation (1). At low pressures in inert gases where β is small the impact on the cathode or ultraviolet activity may be the predominating action, as the number of gas molecules ionized by positive ions would be small. According to Townsend the action as pictured under equation (4) is definitely borne out at very low pressures by the work of Villard.[10] The independence of the sparking potential of electrode material at low and high pressures in other gases could be ascribed to adsorbed gas or oxide films. In fact, as has recently been shown by Jackson,[19] the energy required to remove electrons from a metal surface by impact with positive ions *increases* markedly with the degree of gas denudation of the metal surface and becomes characteristic of the metal surface. With less carefully cleaned electrodes, Baerwald [20] and others have found that electrons were liberated by impact with positive ions having energies as low as 10–20 volts and in these cases the nature of the electrode material made relatively little difference in the number of electrons liberated. That is, while for clean surfaces the cathode material may play an important rôle, this need not be the case even if electrons are generated by positive ions when the electrode material is in an atmosphere such as air at atmospheric pressure. Hence thus far there is no contradiction between the two points of view.

Holst and Oosterhuis [2] originally pointed out another really serious difficulty with the theory. They showed that from the experiments of Pidduck [21] in He a value of β/p was found in which the energy which the positive ion acquired on a mean free path from the fields could only be a small fraction of the ionizing energy for a gas molecule. It was also only a small fraction of the energy of 10–20 volts observed by Baerwald as necessary to cause an observable (say 1 per cent.) electron emission on bombardment of the cathode. Thus values of β/p were observed where a β/p as interpreted by Townsend could not exist. A similar discrepancy had been noted by the writer for the case of sparking in air at

atmospheric pressure. Thus the sparking potential in air gives X/p as about 40. Values of β/p with such values of X/p in air could not be measured by Townsend or his pupils, as they were too small. In fact in air values of β/p were not obtained below an X/p of about 200. The β/p then could hardly account for sparking in air at atmospheric pressure and yet Townsend's theory of the breakdown depends on just this factor. Theoretically in spite of the fact that according to Hurst[38] it is not possible to express β/p as a $\phi(X/p)$ by a relation analogous to that derived by Townsend for electrons, one may, to better understand the situation, use it; for theoretically it is more nearly correct to apply it to ionization by positive ions than to electrons. This relation says that

$$\frac{\beta}{p} = \frac{1}{760\lambda_0} e^{-V_0/760\lambda_0(X/p)}.$$

Here λ_0 is the mean free path of the positive ion in the gas, and V_0 is the ionizing energy needed by the positive ion to ionize a molecule. One can assume the most favorable case and set λ_0 equal to that for a molecule of the gas and V_0 as 10 volts. Actually the experiments of Loeb and Yen[22] on mobilities of ions in high fields (X/p up to 20) indicate that λ_0 is at least 1/4 of the molecular free path, and Franck[39] would set the ionizing energy for molecular ions as at least 1.5 times that for electrons. The favorable assumptions lead to the following values of β/p as a function of X/p.

$\frac{X}{p}$	200	100	50
$\frac{\beta}{p}$	0.166	2.05×10^{-4}	3.17×10^{-10}

It is seen that β/p could hardly be expected to reach values sufficient to account for the spark under values of X/p of 100, even granted a considerable persistence of velocity and a fairly long mean free path. Such values would require a field of 150,000 volts per cm., over a few mean free paths, at least, and they probably would require even greater fields than this.

This is indeed a serious discrepancy. It seems strange that it has never been clearly recognized before, although it seems probable that other workers have been vaguely conscious of it.

The criticism is probably not valid in *the one clear case where β must be active* (i.e. that of positive point discharge), because in these phenomena the fields about the point are far higher than for a spark between plane parallel electrodes (e.g. in the hundreds of thousands of volts per cm., and possibly even higher around the irregular spots on the points). Here then the positive ions may reach the ionizing energy in the field. The existence of fields even higher than those computed for the points in point discharge is borne out by the nature of the streamers observed in brush discharges as well as from such experiments as Millikan and Eyring's [16] work on the "Pulling Electrons out of Metals," where the currents originated in microscopic, or possibly submicroscopic, spots on the wire where the field was very high.

The criticism of Holst and Oosterhuis is equally applicable to the use of the factor β for liberation of electrons on bombardment of the cathode by positive ions, for this effect has a threshold value of from 10 to 20 volts energy of the positive ions according to Baerwald,[20] and much higher according to later workers. To get around this difficulty Holst and Oosterhuis [2] postulate a new mechanism. They assume that, due to the electrical image forces of the approaching positive ion, electrons are pulled out of the surface, leave the metal, and are not caught by the positive ion. This action would depend on the nature of the metal and *make β independent of the velocity of the positive ions*. An effect of this sort might also be produced by the process of local heating resulting from the neutralization of the positive ion on reaching the cathode in which a thermionic electron escaped.[23] While such actions are doubtless possible, there has so far been *no direct observation of them* in an order of magnitude to help out in this difficulty by any of the many experimental workers. We must therefore regard it as unlikely and look elsewhere

for an explanation. The suggestion of Taylor [3] and J. J. Thomson [24] that the factor β can be due to a photoelectric action on the electrodes or on the gas molecules of the light given out by atoms excited by electrons, and by neutralization of ions produced by the electrons, which would give a way out of this difficulty, is also to be questioned. To anyone who has studied the photoelectric emission of metals such as used for electrodes in air at atmospheric pressure, even using such an intensely luminous source as the quartz mercury arc, *the notion of an appreciable electron emission (equivalent to 1/1000 the ionization by the electrons), from the dark current preceding a spark, must seem highly improbable.*

Townsend [1] in a recent article quotes an experiment by Gill [25] in which Gill measured the currents from different cathodes (Zn, Cu and Fe plates in air with X/p from 100 to 300) whose sensitivity to ultraviolet light varied in the ratio of 1 to 20 for a constant intensity of light. The factor by which the currents obtained from these plates as a function of X/p was increased, was however the same for both. This would not have been the case had a photoelectric effect from the gas excited by the electrons played the predominating rôle. This is therefore another argument against the photoelectric explanation, though it would not be valid if the photo effect were by chance proportional to the number of positive ions formed, the assumption making an equation containing γ equivalent to one with β'.

Still another possibility has been suggested by Schumann [26] in another connection and personally independently suggested to the writer by Prof. R. B. Brode in a seminar course of the writer's on discharge through gases. The suggestion is that the presence of ions of higher ionizing potentials, ionized by electrons and existing in metastable states present in a gas, may produce electrons throughout the gas in a number that is not vanishingly small compared to the activated atoms, by means of inelastic impacts of the second class, as first found by Franck [27] and later corroborated by Harnwell.[28] Brode showed that such an action leads to an equation similar in

form to that of Townsend but with different constants. This ingenious suggestion may explain the results of Gill and Pidduck in He. It is doubtful whether it is applicable to the case of air, though it is possible.

One is thus faced with the real issue in the case, to wit, that sparks occur by a theoretical mechanism, which, while otherwise sound, cannot occur at atmospheric pressure between plane parallel electrodes *under the conditions assumed to exist.* Furthermore it appears that in He the finite values of β/p occur at such values of p and X/p, as assumed from the potential fall, that a positive ion can practically never get the ionizing energy, thus extending the dilemma even to low pressures. The solution of this difficulty the writer believes lies in another direction, namely, in *seriously questioning the existence of the conditions usually assumed to exist when the spark passes.* That is to say, the theory of Townsend, possibly modified for cathodic electron emission by bombardment by positive ions in certain cases, is in essence correct; but the assumptions as to the values of X/p for sparking and in the evaluation of β/p at low fields, as usually estimated, are incorrect.*

With plane parallel electrodes the sparking voltage in air at 760 mm. is about 30,000 volts for a gap of 1 cm. It is *assumed that the field is uniform and that the field strength can be calculated from the relation* V_0/d (i.e. that V_0/d = 30,000 volts/cm.). What measurements of the field have been made, have obviously been made before the spark occurred; that is, while the dark current had a small finite value. However, as the critical potential is reached the current is altering rapidly, producing the conditions which lead to the spark. The existence of this current, while never yet measured, is indicated by the experiments of Warburg [30] with magnetic fields. As to the fields in this period, it seems that we know practically nothing. As the electrons ionize more and more, an ever

* A similar notion is expressed as a possibility by Schumann,[29] although not emphasized, and the dilemma is passed over by him with a mere mention that such a situation is possible.

increasing positive ion current flows towards the cathode. In such fields little is known about the ion and electron mobilities and one cannot definitely calculate the effects occurring. We do know, however, that the speed of the electrons will probably be of the order of 10^4 times that of the positive ions. A positive space charge will therefore accumulate at the cathode, which, as the conditions for the spark build up, should increase. At the instant of breakdown, therefore, the field at the cathode may, for aught we know, be as much as 300,000 volts per cm., a field which could give energies to the positive ions which enable them to ionize.

The current density necessary to produce such a field at the cathode due to a positive space charge, assuming the electrons swept out as fast as they are generated, can easily be estimated. From the relations

$$\frac{d^2V}{dx^2} = 4\pi\rho e \qquad \text{and} \qquad i = \rho e k \frac{dV}{dx},$$

(where ρ is the ion density, e the charge on an ion, k the mobility of the ion, i the current per cm.^{2} and dV/dx the potential gradient), one can derive the expression, if we neglect the potential drop at the anode, which states

$$\left(\frac{dV}{dx}\right)^3 = \frac{12\pi i}{k} V_0,$$

where V_0 is the potential available. For a drop of 30,000 volts across a centimeter gap, V_0 is 30,000, k is 2 cm./sec. per volt/cm., and we can calculate the current density i to give a field of 300,000 volts per cm. at the cathode as 0.053 amperes per cm.^{2}. This current density seems rather high. When it is realized that the area of cross section of the spark (i.e. of the ionized path) is far less than 1 cm.^{2} (i.e. the ionized path is confined to minute space), the total current need not be so very great to cause the effect. At lower pressures the fields do not need to be so high since at the lower pressures the positive ion can gain the ionizing energy easily over long free paths. This is observed, for the sparking voltage decreases with gas density.

The question naturally arises, can such currents originate from the ionization by electrons alone before β reaches a significant value, and if they can, will these be produced under conditions such that β has not become small enough (due to the value of α) to be caused by the few long free paths occurring before the space charge is formed. These questions are naturally raised by Townsend's criterion for sparking, which says, in order that n/n_0 become infinite, $\alpha = \beta e^{(\alpha-\beta)d}$. If we place $d = 1$, and neglect β in the exponent relative to α, the condition for a spark is that

$$\alpha = \beta e^{\alpha}.$$

It is seen that for a given value of α a definite value of β is needed. Now if one assume the real conditions (λ ion $= \frac{1}{8}\lambda$ molecules, and $V_0 = 15$ volts) governing the gaining of energy from the field by positive ions in a single free path, one has from the equation for β/p, cited above, that $\beta/p = 660e^{-250}$, or $\beta = 5 \times 10^5 e^{-250}$. To allow this value of β to exist when a spark passes, α is found to be given by

$$\alpha = 5 \times 10^5 e^{-250}(e^{\alpha}),$$

whence $\alpha = 247$, and $\alpha/p = 0.325$ at 760 mm. This value was observed by Townsend for air at $X/p = 75$.

On the other hand, to give a field due to space charge of 300,000 volts/cm. ($X/p = 400$) with 30,000 volts across 1 cm., a current of 5.3×10^{-2} amps./cm.^{2} is needed. One may calculate the value of α/p needed to produce this alone. The current i between electrodes 1 cm. apart with an initial ionization current i_0 due to ultraviolet light or other ionization of 10^{-16} amps./cm.^{2} will be given by Townsend's simple equation, $i = i_0 e^{\alpha d}$, or as $d = 1$, $i = i_0 e^{\alpha}$. To give $i = 5.3 \times 10^{-2}$ amperes, we find $5.3 \times 10^{-2} = 10^{-16} e^{\alpha}$. This gives $\alpha = 34.6$, and α/p at 760 mm. as $\alpha/p = .0455$. This corresponds to an X/p from Townsend's extrapolated experimental curve for α/p as a $F(X/p)$ for air of between 40 and 50 (i.e. near the critical sparking value for air).

It is thus clear that at values of X/p near the value for

sparking as actually observed, α/p has already a value that will actually cause a space charge current giving very high fields. Were the fields required lower than 300,000 volts/cm., the value of α/p would be slightly lower still. These values of α/p are about one tenth those required to produce an adequate β/p for ionization by positive ions to occur at $X/p = 40$.

One must then conclude that the mechanism of the spark is as follows. At a value of a uniform field X which is that for sparking, α/p attains a value such that a current density is created sufficient to give a space charge which increases the field near the cathode to from two to ten times the uniform potential gradient originally existing. This gradient rapidly gives values of α/p in the neighborhood of the cathode such that the values of β/p required to cause a spark fall within a region where the positive ions can actually acquire the energy from the field to produce ionization often enough to create the condition for a spark, and the spark passes. Thus all the conclusions arrived at above are reasonable and one must look for a distorted field just before the spark jumps.

The conditions studied indicate that the critical value of α/p (and hence X/p) will depend on i_0, the initial current. The effect of i_0 on α is however slight, and the change of the intensity of the initial current i_0 will produce only a minimal change in the X/p for sparking. Such an influence has been observed in the experiments with ionization and spark lag, where the sparking potential is lowered by some few per cent. (one or two) by the introduction of ionization in the space. It is also possible that a small local increase in n_0 at some region of the space between the plane parallel electrodes will localize the building up of the high field along a given line. The spark will therefore be localized in space, as is the actual case.

A proof for the existence of such a condition is given by the observations of Holst and Oosterhuis [2] in Neon at low pressures. At currents of 10^{-9} amperes they observed *a visible glow over the anode with a dark space at the cathode. This*

definitely points to a non-uniform field, and the values of X/p calculated in this case from the assumption of a uniform field would have been seriously in error. It is also probable that just such conditions occurred in Gill's work in He and in other gases. He found a variation of β/p with an assumed X/p which may not have been the true X/p. If the true X/p had varied proportionally with the assumed X/p, a condition which could have occurred within the narrow limits of observation, a regular variation of β/p with the X/p assumed would have been observed, but not the true relation.

In a recent paper by Compton [31] and Morse, it has been shown that in the arc an automatic adjustment of fields occurs, giving the optimum conditions. It may be that some such adjustment also occurs in the building up of the field, and that these conditions give the agreement found by Gill and Pidduck.

These variations of β/p with X/p might possibly also be explained more readily by the mechanism suggested by Brode.

The writer feels that a careful investigation of the currents and fields preceding sparks should be made by oscillographic methods for estimating the current within 10^{-5} seconds before breakdown. At present he sees no way of doing this simply, and there seems to be no way of measuring the field strength at breakdown near the cathode. It is possible that laboratories equipped for oscillographic work might undertake such an investigation, and should do so.

The existence of high fields preceding breakdown demands investigation of a condition hitherto neglected except in a recent paper by Rogowski.[32] This is the existence of a *true spark lag*. This is the time interval taken to build up the field at the cathode. It will be an interval of not too great a value, say of the order of 10^{-4} seconds, which is the time taken for positive ions to move a few centimeters in the field. Up to now no definite spark lag interval [33] has been experimentally established as acceptable. Zuber [34] did not observe a regular period of this order. Pedersen,[35] using rather

indirect methods and over-voltages, observed one of the order of 10^{-7} seconds. The work on the effect of frequency [36, 37] on sparking definitely indicates the effect of the time on the potential due to the failure of the field to remove positive ions and electrons between cycles, because of the inertias of the ions and electrons. They however furnish little assistance in the present question. It seems therefore quite urgent that some attempt be made by a new method to detect the true time lag of sparking which must be involved in building up the non-uniform fields which are demanded by the theory.

In conclusion, then, one may say that in general the theory of the spark proposed by Townsend is correct with the possible addition of a factor accounting for electron emission from the cathode by positive ion bombardment, which is a dominating one under certain conditions. *In addition, however, one must recognize the probable inaccuracy made in assuming that the breakdown field strength can be calculated as for a uniform field,* since it is probably not a uniform field at all, even in the case of infinite plane parallel electrodes at the time of breakdown.

SUMMARY.

After a brief summary and discussion of Townsend's theory of the electrical breakdown of gases in the light of certain recent criticisms, it is shown that these are compatible with his theory except for one serious discrepancy. This discrepancy lies in the fact that at the assumed fields at which breakdown occurs in air, at atmospheric pressure, and in inert gases at low pressures, the β/p of Townsend's theory cannot have the significance given it by Townsend, as the positive ions are incapable at these fields of acquiring the ionizing energy. Various solutions proposed are discussed and found inadequate. It seems necessary, in order to keep this otherwise successful theory, to doubt the validity of the assumption generally made for plane parallel electrodes, that the potential drop between the plates is uniform before the spark passes. If fields about ten times as great as those calculated from the uniform drop existed, the theory could be applied. It is shown that such fields are possible under the conditions of

the spark potential experiments, due to space charges resulting from the difference of ionic and electronic velocities. The existence of such fields requires a finite spark lag interval of about 10^{-4} seconds, as yet not definitely observed. Experiments should be undertaken in laboratories equipped for the purpose to look for both the spark lag and the non-uniform field.

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PHYSICAL LABORATORY,
UNIVERSITY OF CALIFORNIA,
BERKELEY, CALIFORNIA,
November 24, 1927.

Illicit Liquor. H. V. ATKINSON of the State University of Iowa (*Journ. Am. Pharm. Asso.*, 1928, *17*, 28–35) divides illicit liquor into three classes:—(1) Properly made whisky and brandy which are either smuggled or obtained from domestic storage, (2) synthetic liquors, and (3) moonshine or home-brew liquors. The first class produces harmful effects only as the result of an overdose; the margin between the concentration of alcohol in the blood producing intoxication and that producing collapse is not great. In the second class, the alcohol content may be derived from denatured alcohol; and all or a portion of the denaturant may still be present and exert its toxic action. Methyl alcohol is the denaturant which has been reported most frequently as the cause of this type of poisoning. Continued use of poorly made liquors of the third class injures the kidneys, even in the absence of acute symptoms. The compound which produces this injury has not been determined, but allyl alcohol is suspected. If poorly made liquors contain excessive amounts of the "heads" and "tails" of the distillate, they may possibly be poisonous as the result of the presence of excessive amounts of acetaldehyde and fusil oil. A few cases of lead poisoning have been recorded, due to the presence in the liquor of lead derived from the solder in the joints of the still.

J. S. H.

Separation of Arsenic from Antimony. LEROY W. MCCAY of Princeton University (*Journ. Am. Chem. Soc.*, 1928, *50*, 368–373) has devised the following procedure for the quantitative separation of arsenic from antimony. A solution of arsenic acid and antimonic acid in moderately dilute hydrofluoric acid is rendered slightly, but distinctly, alkaline by addition of ammonia; then an excess of silver nitrate is added; the arsenic is precipitated completely as silver arsenate, free from antimony. The precipitate is collected on a filter; and the antimony is present in the filtrate.

J. S. H.

Atomic Weight of Titanium. GREGORY PAUL BAXTER and ALBERT QUIGG BUTLER of Harvard University (*Journ. Am. Chem. Soc.*, 1928, *50*, 408–415) report that analyses of titanium tetrachloride and titanium tetrabromide agree in yielding the value 47.90 for the atomic weight of titanium. J. S. H.

THE NEW QUANTUM DYNAMICS.*

BY

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SPEAKING of physical theories, a mathematician of eminence has said, "God made the integers and man made all else." Now I am not sure that man did not make the integers too; but without invoking this additional point of dispute we doubtless have in the statement of the eminent mathematician referred to a point of view sufficiently strong to divide thinkers into two classes whose attitudes towards nature differ almost to the point of acrimony.

A few hundred years ago, the only thing which satisfied the average mind in the matter of an explanation of phenomena was an appeal to the supposed actions of some being or group of beings. The sun went around the earth because it was carried by angels. At this stage the mind was content. If we had raised the question of why the angels wanted to carry the sun around the earth, the probable answer would have been to the effect that it was not for us to inquire into that, the modern paraphrase of which statement is "Well, we must assume something, we must take something for granted, we must start somewhere." Now it is in the difference in our conceptions of this "somewhere" from which we are willing to start that is to be found the root of most of those troubles in which one thinker will contend that the theory of some other is meaningless to him. Each individual or class of individuals has certain criteria (usually unconscious ones) which determine for him whether or not he will be satisfied with a certain type of theory. If the theory starts

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out from his own "somewhere," all is well. Possibly he is content if it proceeds to discuss nature in terms of masses interacting with each other through an elastic medium. If the elastic properties which it is necessary to attribute to the medium are very like those of water, he will be more than content. He will be happy. You may convince him that, in the sense in which he uses the word "understand," he does not understand the reason why water acts as it does. You may rob him of his philosophy, but you cannot rob him of his happiness. He will shrug his shoulders and say, "You are talking metaphysics. We must start somewhere." You may even get him to admit that he has no reason to be happy, but he will be happy all the same. Once he is happy, he stays that way, regardless of any attempt on your part to show him that he ought to be morose.

If now you make some different starting point, a starting point which says that the orbits of the planets are a set of curves which make a certain integral a minimum, or something equally abstruse, our friend will assert that your statement has no physical significance. He will say it doesn't mean anything. "What," says he, "is the reason for starting there?" You may ask him what the significance was of starting where he started, and remind him of his confession that he could not justify it. As a last resort he will then probably make that ultimate personal appeal to his own anatomy in assuring you that he feels in his bones that such and such is the case, and beyond his bones he will not go.

Now he who would understand the point of view of the new mechanics of the quantum theory need not get to the point of feeling in his bones that it is true, but he must get to feel in those bones that there was nothing to the feeling which he may formerly have had in his bones as to the fundamentality of the older views. Those older points of view were built up by analogy with the behavior of the large scale phenomena of life which presented themselves to us in our youth and became ingrained in our consciousness as natural before we had reached the age at which we might question their necessity.

We have become accustomed to talk of natural phenomena in terms of what we call space and time. We see a planet in the sky, and we attach to it four numbers x_1, y_1, z_1, t_1, which, we say, represent its position at a certain time. We observe the sun, and we assign to it four numbers x_2, y_2, z_2, t_2. Possibly we observe also a comet, and we assign another set of numbers x_3, y_3, z_3, t_3. It is usually convenient to assign the same t number to all of the bodies. We then write down equations between these various numbers x, y, z, expressing how they are to be related to each other and to t. In other words, we make one assignment of the numbers and our equations serve to specify another assignment, and another, and so on. These equations usually take the form of differential equations. Newton's law of gravitation is an example. The equations will give us all the values of x, y, z associated with any value of t we like to assign provided that we give a sufficient amount of additional information. If the equations are of the first order, we have to assign the values of x, y, z associated with one value of t. If they are of the second order, we have to assign, in addition, numbers for the first time derivatives for that value of t. The equations when solved may then predict that, for a certain value of t, $x_1 = x_3$, $y_1 = y_3$, $z_1 = z_3$. We say that the comet collides with the planet. I wish to suppress the idea of x, y, z having anything to do with distances. They are numbers, and the object of our astronomer is to build up a set of equations and to assign a suitable set of numbers for one value of t so that there shall be as many flashes of light in the sky (collisions) as there are subsequently found pairs of xyz's which come equal for equal values of t. But you will say that the astronomer actually measures x, y, z. True, but he need not do so. He might just sit in his chair and assign sets of numbers to the various things he sees, set up equations and predict collisions. If what he predicts does not happen, he must try other equations, or assign different sets of numbers as associated with one value of t. One might think he would have a merry time for the rest of eternity trying to get the

thing right in this way. But matters would not be so bad if he had more or less of a periodic structure to deal with, such as we have in the atom. In fact, even in the Bohr-Sommerfeld theory of the atom, the problem is exactly that of our astronomer with folded arms. It is possible to broaden our astronomer's interests beyond the mere observation of celestial catastrophes. He might attempt to build up his theory in such a way that if any of his values of x, y, z became infinite for all assigned t's greater than a certain number, he would eventually lose sight of the planet and never see it again. An analogous phenomenon in the case of the atom is the ejection of a beta particle from the atom. But you will say our astronomer does vitally interest himself in the time at which the collision occurs. That is true. He has a great respect for the grandfather clock in his office. The position of its hands determines his going to lunch, the grayness of his hair as old age creeps on, and so forth, so he assigns to the planet for the t numbers the numbers on the clock, and tries to include in his theory a little more than is involved in the mere prediction of celestial catastrophes by trying to arrange matters so that if two values of x, y, z become equal for a certain t, it shall result that on seeing the flash in the sky and immediately looking at the clock he shall see there, not the number t, but another number which he can calculate on the basis of an idea at the back of his head concerned with something about the times taken for light to get from the collision to his eye and from the clock to his eye. In an analogous way we may say that he interests himself in the orbit of a planet quite apart from any celestial catastrophe. To the astronomer the orbit is a relation between the x, y, z numbers for one of the planets. A transformation of all the coördinates would not affect the story of the celestial catastrophes but it would affect the equations of the orbits and he is interested in the equations. The equation of the orbit has meaning to our astronomer only in the sense of a definite scheme of assignment of the numbers. On his photographic plate the astronomer sees a curve, and he describes it in

terms of equations between numbers representing measurements made from the edges of the plate with meter scales (cartesian measures), or in terms of some other method specified with sufficient definiteness to have meaning to him. Then, in terms of these numbers he calculates the numbers to be assigned to the planet in the sky. His process for doing this involves many things concerned with his theory of how the image got on to the plate, his theory of light, of his optical instruments, etc. In these we are not interested. All that we desire to know is that for the totality of all the numbers on all his photographic plates, and we may as well add the corresponding readings of his clock,[2] he has a definite way of assigning numbers to the planets in the sky. It is in terms of these numbers that he endeavors to establish equations, and calculate orbits, from which, by an inverse transformation of the numbers, he may deduce the numbers appropriate to the photographic plates and in terms of these reconstruct the curves which he observes on the plates. If our astronomer, using measuring rods, should plot perpendicular distances proportional to x, y, z in his laboratory, he will obtain a sort of model of the orbit. It is by no means a foregone conclusion that if he looks at this model it will produce via his retina the same mental impression as did the planet itself. That it does so, at any rate approximately, is something bound up with the way the light behaves in coming from the model to the eye.

Now in the last analysis, even the orbit as pictured on a photographic plate attached to a telescope is really to be described as corresponding to the successive coincidences of the image of the planet with certain identifiable points of the plate. In its purest sense, the procedure is to find a way of attaching to the planets numbers with relations between them, find a way of fixing in terms of them the numbers to be assigned to the image of one of the planets and find a way of assigning numbers to the points on the photographic plate in such a manner that if the four numbers for the planet's

[2] To incorporate the time numbers we must imagine the astronomer to actually observe the formation of the images on his plates.

image become equal to the four numbers for the photographic plate the point in question will be found blackened. In practice, the procedure is a little less cold blooded, and things are imported into the description of the phenomena which are not strictly involved in the phenomena themselves, even as a writer will put into his penmanship flourishes which play no part in the ideas expressed.

I have dilated upon this matter in order to enforce the idea that what is fundamental in the process of describing observations in the way we are discussing is the assignment to the various objects of numbers with relations between them expressed in the form of so-called laws. The fact that we sometimes assign these numbers as a result of certain measurements and calculations with scales, telescopes, and clocks is in many cases quite incidental. Sometimes we meet the statement that lengths of the order of the diameter of an electron are meaningless since no rod can be shorter than an electron. This is true, but there is definite meaning to the assignment of numbers to entities which figure in our theories, however small the differences of pairs of numbers may be.

You will observe that I have talked of the assignment of four numbers x, y, z, and t. Why four? Well, simply because we find that with four numbers assigned to each entity we can express relations which will correctly predict an astronomical catastrophe as one symbolized by two sets of four numbers becoming equal. Herein is to be found an illustration of the ultimate meaning of space being three-dimensional, or four-dimensional if we include t. It might have been necessary to have had five numbers for each planet in order that the laws we wrote down could be made to predict correctly the catastrophes as corresponding to the equality of all five numbers. If we tried to get on with only x, y, and t, we should occasionally have x and y equal and observe no catastrophe. The comet would be merely "behind the planet" as pictured in our three-dimensional space sense, and one who had never seen the planets except projected on a screen might well wonder why, when there was an eclipse

of one by the other, there was sometimes a catastrophe and sometimes not. As a matter of fact our actual instantaneous visual picture of the universe is to all intents and purposes a two-dimensional one in view of the smallness of our stereoscopic vision in relation to the distances of the planets.

It is perhaps well that we do not form too firmly the idea that the only way of predicting the observed phenomena is by the assignment of numbers to the planets, for example. It is not inconceivable that we might fit up our laws in terms of appropriate names assigned to the entities which we discussed, and discuss collisions in terms of suitably assigned relationships between these names. Numbers are things rich in law, however, and their assignment to the entities provides us with a means of expressing relationships of a very far-reaching kind.

Going out of the realms of astronomy into atomic physics, we again find a procedure in thought somewhat similar to that above sketched. Here the entities to which we assign the numbers are not visible. They are protons, electrons, quanta, and so forth. We now have to postulate our entities as well as assign numbers to them. We postulate them in the filament of the X-ray tube. They are the electrons. We find that our results depend upon the nature of the target of the tube, so we postulate entities in the target. The effects on the photographic plate depend upon the presence and nature of the crystal in the X-ray spectrometer, so we postulate entities there. Finally we postulate entities in the photographic plate. We assign numbers to each of these entities. Custom urges us to assign four to each. Our electromagnetic equations, our quantum theories, and so forth, are the means of expressing relationships between the numbers assigned to the entities. We are happy when we can get along by assigning four numbers to each entity. We say that all is explained in terms of the motion of the entities (protons, electrons, etc.) in space. We are further happy if there is such symmetry in the relationships as to correspond to there being only a limited number of different kinds of

entities—nothing but protons and electrons, and possibly quanta. If we have to assign more than four numbers to any of the entities in order to tell our complete story, we regard the entity as complex. It is not yet fully understood, and we shall hope some time to be able to replace it by a number of other entities each with only four numbers assigned. The appearance of a line (or let us say a point on a line) on the photographic plate is now to be predicted by our theory as the result of two sets of four numbers for certain of our entities becoming equal. One set refers to a silver grain on the plate and another set to a quantum—the quantum which hit the silver grain in question. Not always do we seek in a collision the symbol of an event. It may be a mere specified change in the numbers associated with an entity (an electron in the silver grain) which serves as our point of correlation with experience. We calculate that as a result of the electrons from the filament, of the target, and of the calcite crystal, a certain entity in the photographic plate has its numbers changed outside of certain limits which in the absence of these agents it would have occupied—in other words we say that a photoelectric effect has been produced in a certain silver grain, and at this point we make our correlation with the photographic action.

The procedure we have sketched for discussing phenomena is of course not the only one possible. Thus in formal contrast to it we have the procedure in which, instead of talking in terms of entities with numbers attached, we divide space into volume elements and assign to these elements quantities, such as charge density and current density, which are allowed to vary with a certain parameter t associated with the volume element, the variation being controlled in general by certain specified laws. The numbers and their variations are utilized for the purpose of defining quantities (electric fields, for example) for the whole space, and these fields in turn control in some specified way the variations with t of the magnitudes in the volume elements in their respective vicinities. Thus, for example, the field due to

the motion of, say, an electron considered as a volume distribution of charge will, through suitable laws, control the motion of another electron considered again as a volume distribution of charge. The point of contact of all such theories with observable natural phenomena is nearly always sought, however, through the realization of a condition in which the numbers assigned to the volume elements have appreciable values only in a limited region at any one value of t, as in the case of the electron considered formally as a volume distribution extending throughout all space but in practice highly concentrated near a single point. In such cases the entity functions practically as a point entity or at any rate it functions as an isolated entity in the matter of setting up a correlation with the observable phenomena, as for example when its ejection from a molecule of the photographic plate is the signal of photographic action.

The foregoing procedure may appear cold and heartless as I have described it. But, if we concentrate attention on what we actually do as distinct from pictures in our minds, we have to admit that the procedure is as cold blooded as I have sketched. Of course there is no harm in the pictures so long as they do not introduce unnecessary difficulties of their own—difficulties not pertinent to the discussion. We shall do no harm in representing the electron by a red chalk dot on the board so long as we do not afterwards trouble ourselves about the consistency of supposing an electron to be red since if it were so it should become much more agitated in red light than in blue.

THE BOHR-SOMMERFELD THEORY.

During the last fifteen years, our powers to think intelligently about the atom have been aided by one of the most epoch-making advances ever made in physics, by the Bohr-Sommerfeld theory of atomic structure. Let us recall for a moment a few of the salient features of this theory.

We think of a definite mechanical structure with a nucleus and electrons revolving around it. The motions of the system are supposed to be controlled by the ordinary laws

of mechanics which, for the purposes in hand, are most conveniently employed in the form of the Hamiltonian equations

$$\dot{p}_r = -\frac{\partial H}{\partial q_r}, \qquad \dot{q}_r = \frac{\partial H}{\partial p_r}, \tag{1}$$

$$H = H(q_1, q_2, \cdots; p_1, p_2, \cdots),$$

where H is a function of the coördinates q_r and of the momenta p_r and is, in the case of a conservative system, simply the energy of the system. The Hamiltonian equations guard, of course, the constancy of the energy for any one path, in cases where H does not explicitly involve the time.

It is postulated that not all of the orbits which these equations would permit can exist, but only those for which, in a suitably chosen system of coördinates, the values of $\oint p_r dq_r$ taken around cycles for the various degrees of freedom have values equal to integer multiples of h, $\oint p_1 dq_1 = n_1 h$; $\oint p_2 dq_2 = n_2 h$, $\cdots$. If we fix the integer values which we assign to the integrals pdq, we fix the orbits and so the value of H. The value of H so determined will be a function of n_1, n_2, n_3, etc., and of course of h and of the fundamental constants of the structure, such as electronic and nuclear charge, to be found in the Hamiltonian function. The form of the energy as a function of the n's will be controlled by the form of the original Hamiltonian function H. We then postulate that the frequencies which may be emitted by the atom are given by

$$h\nu = H(m_1, m_2, \cdots) - H(n_1, n_2, \cdots), \tag{2}$$

where $H(m_1, m_2, m_3$, etc.) is the value of H for one set of the assigned integers n_1, n_2, etc., and $H(n_1, n_2$, etc.) is the value for another set of the assigned integers. It is supposed that the energy emitted in the frequency corresponding to a transition from state H_1 to H_2 is equal to $H_1 - H_2$, but nothing is said as to the relative numbers of times which the various frequencies are emitted in any interval. In other words, nothing is said as to the intensities.

The mind has a very definite picture of everything that is

occurring other than the picture of the emission of the radiation itself. It sees the various electrons describing their orbits and, in the change from one value of H to another, it sees an electron jump from one orbit to another, or it may see several electrons jump from one possible set of orbits to another possible set, there to remain in stable equilibrium until another such jump occurs.

In spite of the vividness of the picture, it is possible to express the complete content of the theory in so far as its application to spectra is concerned without any picture, and without even utilizing the Hamiltonian equations, or even acknowledging the existence of any physical quantities to correspond to the momenta. Those familiar with the technique of the mathematical procedure will recall that the determination of the energy levels does not necessitate the calculation of the orbits. The argument in fact runs something like this: Suppose that

$$H = H(q_1, q_2, \cdots; p_1, p_2, \cdots) \tag{2a}$$

is the appropriate Hamiltonian function for some atom. Let us write down the equation

$$H\left(q_1, q_2, \cdots; \frac{\partial F}{\partial q_1}, \frac{\partial F}{\partial q_2}, \cdots\right) = \alpha_1, \tag{3}$$

where $\partial F/\partial q_r$ occupies the position formerly occupied by p_r, and where α_1 is a constant. It is known that the p_r which are solutions of the Hamiltonian equations are given by

$$p_r = \frac{\partial F}{\partial q_r}, \tag{4}$$

where F is a solution of (3). F is a function of the q_r and of $N - 1$ other arbitrary constants $\alpha_2, \alpha_3, \cdots, \alpha_N$, if there are N degrees of freedom. There is an additional arbitrary constant which is simply additive to F and which can be ignored for all purposes in hand.

Only in the case where F comes out of the form

$$F = F_1(q_1, \alpha_1, \alpha_2, \cdots, \alpha_N) + F_2(q_2, \alpha_1, \alpha_2, \cdots, \alpha_N) + F_r(q_r, \alpha_1, \cdots, \alpha_N) + \cdots, \tag{5}$$

where F_r involves only q_r and $\alpha_1, \alpha_2, \cdots, \alpha_N$, do the Sommerfeld quantization rules have meaning, and in those cases

$$n_r h = \oint p_r dq_r = \oint \frac{\partial F_r}{\partial q_r} dq_r, \tag{6}$$

so that $n_r h$ = change of F_r in going around a cycle in the sense ordinarily understood in the quantum theory

$$n_r h = \varphi(\alpha_1, \alpha_2, \cdots, \alpha_N), \tag{7}$$

where φ is a known function determined ultimately by the form of H. There are N equations like (7), and these serve to determine the N α's, as functions of the assigned quantum numbers n_1, n_2, n_N, and among them becomes determined the energy α_1 as a function of the integer numbers n_1, n_2, etc.

Now if starting with the mathematical form specified in H we content ourselves with simply describing the mathematical procedure by which we arrive at the values of H to be admitted, and if we are willing to omit all concepts which are extraneous to the description of this procedure, we may describe the procedure as follows:

(1) Write down a certain Hamiltonian form

$$H\left(q_1, q_2, \cdots; \frac{\partial F}{\partial q_1}, \frac{\partial F}{\partial q_2}, \cdots\right),$$

which, while it is *suggested* by the form with the p's in it, may be regarded as our starting point.

(2) Put α_1 equal to this expression.

(3) Seek a solution for F of the form (5) where $\alpha_2, \alpha_3, \cdots$, etc., constitute $N - 1$ constants of integration.

(4) If the solution has been successfully found, write

$$\begin{aligned}
n_1 h &= \Delta F_1(q_1, \alpha_1, \alpha_2, \cdots, \alpha_N) = \varphi_1(\alpha_1, \alpha_2, \cdots, \alpha_N),\\
n_2 h &= \Delta F_2(q_2, \alpha_1, \alpha_2, \cdots, \alpha_N) = \varphi_2(\alpha_1, \alpha_2, \cdots, \alpha_N),\\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\\
n_r h &= \Delta F_r(q_3, \alpha_1, \alpha_2, \cdots, \alpha_N) = \varphi_3(\alpha_1, \alpha_2, \cdots, \alpha_N),\\
n_N h &= \Delta F_N(q_N, \alpha_1, \alpha_2, \cdots, \alpha_N) = \varphi_N(\alpha_1, \alpha_2, \cdots, \alpha_N),
\end{aligned}$$

where the ΔF's are the changes of F on going through a cycle

of values of the q's in the sense understood in the quantum theory.

(5) Solve these equations for the α's and, in particular, we shall find

$$\alpha_1 = \psi(n_1, n_2, \cdots, n_N), \tag{8}$$

which gives the possible values of a constant associated with the atom as the values obtained by assigning successively all integer values from 1 to N for the various n_r in (8).

As a matter of fact, with the process stated in this way, there is no real reason for speaking of the values of α_1 as energies since we have made no mention of dynamics. We may content ourselves by saying that a certain procedure has been set up for fixing the possible values of a certain constant associated with the atom, the importance of a knowledge of these values being inherent in the postulate that the difference between any two of them when divided by h gives a possible frequency emitted by the atom. In this abstract method of presentation there is nothing to necessitate the idea of quantum jumps. We are not driven to believe that one value of α_1 exists at one time and another value at another. We have simply set up a method of calculating certain constants which we are to associate with the atom. I lay stress on this point because, as we shall see in the case of Schrödinger's theory, the determination of these constants to be associated with the atom is made without the necessity of introducing any idea as to dynamical motions and the like, and it will help us in the understanding of the process there utilized if we can see how an analogous abstract procedure could have been adopted in such a manner as to give the exact equivalent of the Bohr-Sommerfeld theory.

Of course, even though in a lordly way we disown the momenta and the Hamiltonian equations in our abstract procedure for assigning the values of H, it is nevertheless true that if we should *define* a set of quantities p_r by

$$p_r \equiv \frac{\partial F}{\partial q_r}$$

these quantities p_r in collaboration with the quantities q_r would satisfy equations (1) where H has the form 2*a*. This does not of course mean that there are of necessity electrons, or the like, with coördinates q and momenta p, but it does cast a shade upon the aristocratic dignity of the procedure that, even in the sense of mathematical existence, there should be found these p_r with their materialistic rules of behavior, the Hamiltonian equations, capable of singing the same song as the more abstract Hamilton Jacobi equation.

The Correspondence Principle; Line Intensities, etc.—As I have remarked, the Bohr-Sommerfeld theory in the form I have sketched it and without further hypothesis tells us nothing about the intensities of the frequencies emitted. If it had been a fact that the frequencies with which the theory concerned itself were those obtained by acknowledging the orbital motion picture of the atom and analyzing that motion into the appropriate Fourier components, the dynamical theory would have had power to tell the story of the relative amplitudes, polarizations, for the different frequencies.

Now it is a mathematical fact that the quantum theory frequencies, which are emitted as a result of relatively small changes in high quantum numbers, do have a close relation to the classical frequencies, fundamentals, and harmonics which correspond to the orbital motions. Thus, for example, in the case of a simple periodic motion, the frequency of the fundamental of the orbital motion is equal to the frequency of a single quantum jump in the vicinity of the corresponding energy level, and the frequency of the sth harmonic is equal to the frequency of an s quantum jump in that vicinity. This does not mean of course that the quantum process of emission merges into the classical for large quantum numbers. The processes of emission may here be as different as they are for small quantum numbers. Nevertheless, the mathematical fact that the frequencies correspond at the high quantum numbers led Bohr to postulate that a like relation held between the intensities, a postulate which was very successful in correctly predicting zero values for the intensities

of certain lines which, without this postulate, might have been expected to appear. Bohr extended this idea of correspondence in a more reserved manner to the amplitudes and polarizations of the light emitted in transitions of small quantum number, relating the intensities, for example, in the case of a jump of p quantum units of a singly periodic system, to the intensities of pth harmonics in the Fourier expansion of the motion in both the initial and the final orbit. The law of correspondence becomes here to some extent empirical. It speaks with greatest definiteness when one of the Fourier harmonics concerned has zero intensity, in which case it predicts zero intensity for the line as actually emitted.

One's philosophical intuition dislikes a process of predicting what would happen in a certain case by setting up a correlation with what we should get in another case if something happened which does not happen. However, the newer theories would be in no position to criticize the correspondence principle on the ground of artificiality alone, for they live in houses of much thinner glass. It is only on the basis of lack of definiteness in its statement that the correspondence principle can be criticized in relation to the newer theories. I think it is worth while to point this out; for, since the parents of the Bohr-Sommerfeld theory were classical and conservative, one is apt to regard radical tendencies in that theory as of much greater seriousness than one would attribute to them in such theories as the matrix theory or that of Schrödinger which were born in radicalism.

Systems More Complicated than Hydrogen.—When we come to systems more complicated than hydrogen, the Bohr-Sommerfeld theory has not been very successful in speaking without ambiguity even as to the frequencies. In the first place we become involved in the difficulties inherent in the problem of many bodies. Even the problem of normal helium is supercomplicated, and for more complicated atoms matters get worse and worse, so that approximations and empiricisms have to be invoked to tell even a partial story.

Now we would not be justified in condemning a theory as

wrong because it is complicated and taxes too greatly the skill of the mathematician. But there is something more serious. For there are indications that even if we were endowed with infinite mathematical skill so that we could take into account all the mutual perturbations of the electrons, the results obtained by calculating these perturbations in the classical way would not be right. The very features which have necessitated the departure from a classical to a quantum theory for the effect of optical frequencies in the photo-electric effect, for example, enter again in rendering uncertain the way to calculate the mutual influences of the electrons of complicated atoms on each other, since these electrons approach and recede from each other in times comparable with the periodic times of light waves.

Then again, while the principle of expressing frequencies as the differences of two functions of integer numbers—the functions representing energies divided by h in the Bohr theory—seems to hold with a very wide degree of generality, a complete accord with experiment seems to require that in certain cases we admit, in addition to integers, half integers. While there is no formal objection to the appearance of these half quantum numbers in the Bohr-Sommerfeld theory, that theory gives no aid to their inclusion by suggesting when they should appear and when they should not.

Finally, one of the most serious difficulties presents itself in the fact that the quantization process involving the integrals pdq becomes without meaning for those cases where the Hamilton Jacobi equation is incapable of solution in any coördinates in a form involving a separation of the variables.

The difficulties inherent in the Bohr-Sommerfeld theory became so acute as to seem, even to its authors, to call for a complete revision of our whole attitude towards atomic processes, so that when only a couple of years ago a new aspect of the whole question was suggested by the work of Heisenberg, Born, and Jordan, it was seized upon with such avidity by all atomic architects that now even the atomic mason and plumber of the atom, the laboratory man, must

take stock in what is going on so that he may acquaint himself with the sort of structure he may be expected to work upon. No sooner had the Born and Heisenberg theory gotten started than a new viewpoint appeared in the work of Schrödinger, a viewpoint which had even a greater appeal to many. These two newcomers have endeavored to settle their rivalry in friendly fashion by finding a sort of blood relationship between them. In fact their godfathers claim that they are almost more than twins.

THE THEORY OF BORN, HEISENBERG AND JORDAN.

In the Born, Heisenberg, Jordan theory the attempt has been made to make known facts rather than atomic models the starting point and to build up a theory under the constant control of those facts.

The first outstanding fact is that the spectra of the atoms seem to be comprised of a number of simple harmonic frequencies.

The second outstanding fact is that the frequencies emitted from any one atom seem to satisfy the condition that they can all be obtained from a suitably assigned number of constants W_1, W_2, for the atom, by writing

$$\nu_{12} = \frac{W_1 - W_2}{h}, \qquad \nu_{13} = \frac{W_1 - W_3}{h}, \qquad \nu_{mn} = \frac{W_m - W_n}{h}.$$

In Bohr's theory we thought of them as corresponding to energy levels but they need not be so in general. Moreover, with a few exceptions for which it seems to be possible to provide rules as to their occurrence, all of the lines predicted by the various differences of the W's occur. The latter remark is of some importance, because, if we were given any assigned set of frequencies, we could always assign a set of W's which in their differences would contain all these frequencies. In general, however, the W's having been assigned, they would predict a lot more frequencies than they were designed to predict. The totality of frequencies calculable from n W's is $n!$ and the essential feature of our remark is that the number n of W's which it is necessary to assign to

account for N frequencies is just such that $N = n!$, after making allowance for certain absent frequencies in respect of which it may be said that one of the most powerful features of the newer theories is their power to predict zero intensities for these frequencies.[3] The law of course contains the principle that

$$\nu_{12} + \nu_{23} = \frac{W_1 - W_2}{h} + \frac{W_2 - W_3}{h} = \frac{W_1 - W_3}{h} = \nu_{13},$$

and in general

$$\nu_{nk} + \nu_{km} = \nu_{nm},$$

which is the Rydberg-Ritz combination principle. Everything so far may be regarded as based upon experimental fact.

Let us write down the frequencies in a two-dimensional array for the purpose of looking at them.

$$\begin{array}{ccccc}
\nu_{11} & \nu_{12} & \nu_{13} & \nu_{14} & \cdots \\
\nu_{21} & \nu_{22} & \nu_{23} & \nu_{24} & \cdots \\
\nu_{31} & \nu_{32} & \nu_{33} & \nu_{34} & \cdots \\
\nu_{41} & \nu_{42} & \nu_{43} & \nu_{44} & \cdots . \\
\cdot & \cdot & \cdot & \cdot & \cdot \quad \cdot
\end{array}$$

Of course we have here written ν_{12} and ν_{21} for example. It would appear that we have included a number of impossible frequencies which are negative. This difficulty will disappear if we generalize our idea of frequency a little, and regard ν not as the number of vibrations per second but as the coefficient of $2\pi t$ in the expression

$$y = y_0 \cos (2\pi\nu t - \delta)$$

for a simple harmonic motion. There is now a perfectly definite meaning to ν being negative. For, if $\nu = -|\nu|$,

$$\begin{aligned} y &= y_0 \cos \{-(2\pi|\nu|t + \delta)\} \\ &= y_0 \cos (2\pi|\nu|t + \delta), \end{aligned}$$

[3] I speak here as though there were a finite number of W's for each atom, whereas in practice there is an infinite number. The principle would have definite meaning for an infinite number, and the reader will have no difficulty in suitably modifying the phraseology to apply to such a case.

which is still a harmonic motion. We then think of a set of simple harmonic oscillations

$$y_{mn} = |\bar{q}_{mn}| \cos (2\pi\nu_{mn}t + \delta_{mn})$$
$$= \tfrac{1}{2}|\bar{q}_{mn}| \{e^{2\pi i\nu_{mn}t + i\delta_{mn}} + e^{-2\pi i\nu_{mn}t - i\delta_{mn}}\}$$

which we can write

$$y_{mn} = \tfrac{1}{2}\bar{q}_{mn}e^{2\pi i\nu_{mn}t} + \tfrac{1}{2}\bar{q}_{nm}e^{2\pi i\nu_{nm}t}$$

provided that ν_{nm} is equal to $-\nu_{mn}$, as will indeed be true, according to our specification of the ν's in terms of the W's, and provided that $\bar{q}_{nm}$ is the conjugate of $\bar{q}_{mn}$. We then transfer our attention to the set of quantities $q_{mn} = \bar{q}_{mn}e^{2\pi i\nu_{mn}t}$ which we can, if we wish, write out in an array such as

$$\begin{array}{ccccccc} q_{11} & q_{12} & q_{13} & q_{14} & q_{15} & \cdots \\ q_{21} & q_{22} & q_{23} & q_{24} & q_{25} & \cdots \\ q_{31} & q_{32} & q_{33} & q_{34} & q_{35} & \cdots \\ q_{41} & q_{42} & q_{43} & q_{44} & q_{45} & \cdots, \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \end{array}$$

always, of course, with the understanding that q_{mn} is of the form

$$q_{mn} = \bar{q}_{mn}e^{2\pi i\nu_{mn}t} \tag{10}$$

and subject to the condition that

$$\bar{q}_{nm} \text{ is the conjugate of } \bar{q}_{mn} \tag{11}$$

and that the ν_{mn} are obtainable from a system of W's in such a way that

$$\nu_{mn} = \frac{W_m - W_n}{h}, \tag{12}$$

which of course provides for

$$\nu_{mn} = -\nu_{nm}.$$

Now we want to construct a theory; and what do we mean by that? I might spend an hour dilating upon what we mean by a theory, and should rather like to do so, but for that there is no time. I must content myself by the remark that we all probably think we know what we mean by a theory

even though we may all mean different things. However, we shall all probably agree that we should be happy to see some differential equations for the q_{mn} which would insure that they came out in the form (10), subject to the conditions 11–12. Our happiness would probably be increased if the differential equations had some respectable ancestry.

Matrices.—In preparation for what is to come, I must now pause for a moment to say a few words about matrices, and shall confine myself to the minimum for my purpose.

Let us take a board with a definite number of squares in it, and in each square let us write a number as in Fig. 1.

FIG. 1.

a_{11}	a_{12}	a_{13}	a_{14}	a_{15}				
a_{21}	a_{22}	a_{23}	a_{24}					
a_{31}								
a_{41}								

FIG. 2.

b_{11}	b_{12}	b_{13}	b_{14}	b_{15}				
b_{21}	b_{22}	b_{23}	b_{24}	b_{25}				
b_{31}	b_{32}							

Such a set of numbers is called a matrix. Let us take a similar board with a set of numbers in it as in Fig. 2. I can form a third matrix by writing on another board the sums of the corresponding numbers in the a and the b matrix. This third matrix is called the sum of the other two.

Now from any two matrices A and B, I can form a third matrix in all sorts of different ways. One way, a way which gives a matrix C having very important properties in relation to the matrices A and B, is to proceed as follows:

Let us for example concentrate our attention on the row 2 of the matrix A and the column 4 of the matrix B. Let us build up the quantity

$$a_{21}b_{14} + a_{22}b_{24} + a_{23}b_{34} + a_{24}b_{44} + \cdots, \text{ etc.}$$

You will observe that this is a quantity which is constructed in a certain manner entirely out of the second row of A and the fourth column of B. We write it in the 2, 4 square of another board C; and proceeding in this way with every row of A and column of B we fill up all the squares on our board C and we call the matrix we get the product of the matrix A into B and write it AB. Now if, instead of proceeding with the rows of A and the columns of B, we had proceeded with the rows of B and the columns of A, we should have produced another matrix which we should have no earthly reason to expect equal, term for term, to the matrix we have called AB. Yet, consistency in notation demands that we christen it BA, so that in general we have $AB \neq BA$.

There is nothing of mystery in this. Every appearance of mystery in it comes from the name which has been attached to the operation of constructing the new matrix—the name multiplication. If we had called the process *multuplucation*, nobody would have any difficulty. All suspicion of a difficulty should vanish if we observe that there was nothing which, in its own right, had a prior claim on the term multiplication of matrices until we had defined one. You might say, why not call multiplication of matrices the process of producing from two matrices A and B a new matrix by multiplying the corresponding terms together. There is no formal objection except that the thing so obtained would not have many useful properties.

In general, the mnth term of a matrix ab is given by

$$(ab)_{mn} = \sum_k a_{mk} b_{kn}.$$

Now we observe one very interesting and, as it turns out, important fact as applied to the multiplication of matrices composed of quantities like our q_{mn}. Suppose we have two matrices like the following one composed of the q_{mn}, and

$$\begin{vmatrix} \bar{q}_{11}e^{2\pi i\nu_{11}t} & \bar{q}_{12}e^{2\pi i\nu_{12}t} & \bar{q}_{13}e^{2\pi i\nu_{13}t} \\ \bar{q}_{21}e^{2\pi i\nu_{21}t} & \bar{q}_{22}e^{2\pi i\nu_{22}t} & \bar{q}_{23}e^{2\pi i\nu_{23}t} \\ \bar{q}_{31}e^{2\pi i\nu_{31}t} & \bar{q}_{32}e^{2\pi i\nu_{32}t} & \bar{q}_{33}e^{2\pi i\nu_{33}t} \end{vmatrix} \quad \begin{vmatrix} p_{11}e^{2\pi i\nu_{11}t} & p_{12}e^{2\pi i\nu_{12}t} & p_{13}e^{2\pi i\nu_{13}t} \\ p_{21}e^{2\pi i\nu_{21}t} & p_{22}e^{2\pi i\nu_{22}t} & p_{23}e^{2\pi i\nu_{23}t} \\ p_{31}e^{2\pi i\nu_{31}t} & p_{32}e^{2\pi i\nu_{32}t} & p_{33}e^{2\pi i\nu_{33}t} \end{vmatrix}$$

another composed of other harmonic vibrations $\bar{p}_{mn}$, such that corresponding terms of q_{mn} and p_{mn} have the same frequencies. On forming the matrix product of the q matrix into the p matrix, we shall obtain for the mnth term

$$\sum_k \bar{q}_{mk} e^{2\pi i \nu_{mk} t} \bar{p}_{kn} e^{2\pi i \nu_{kn} t} = \sum_k \bar{q}_{mk} \bar{p}_{kn} e^{2\pi i (\nu_{mk} + \nu_{kn}) t}.$$

If the quantities ν_{mk}, ν_{kn} satisfy the Ritz combination principle (and they will if they are of the form 12), then for every value of k

$$\nu_{mk} + \nu_{kn} = \nu_{mn},$$

so the mnth term of our product matrix will be

$$e^{2\pi i \nu_{mn} t} \sum_k \bar{q}^{\nu mk} \bar{p}_{kn}.$$

You will observe that it also is a complex harmonic vibration and it has the same frequency as the mnth terms of the matrices which are multiplied together. In other words, if we take two matrices with equal frequencies in corresponding squares and multiply them together, we shall obtain a new matrix in which the frequency associated, say with the 2, 3 square, is the same as the frequency associated with each of the 2, 3 squares of the matrices which were multiplied together. Suppose now we take this new matrix and multiply it by another matrix composed of harmonic elements with the same frequencies. We shall obtain a fourth matrix in which again the frequency associated with the mnth term will be the same as it was in all the matrices we have used in its construction.

Now, in ordinary algebra, practically all the functions, let us say of two quantities x and y, are quantities obtained by adding together products of x and y. Thus, our functions of x and y take forms like

$$x^2 + 3y + x^3y$$

and so we define functions of two matrices p and q as a matrix, for example, like

$$p^2 + 3q + p^3q,$$

thereby meaning the matrix we obtain by multiplying the p matrix by itself, then multiplying all of the terms of the q matrix by 3, then multiplying the matrix q by the matrix obtained by multiplying the matrix p^2 by the matrix p, and when this has all been done, adding together the corresponding terms of the matrices obtained. You will observe that in view of the foregoing remark, if the matrices p and q are of the type

$$q_{mn} = \bar{q}_{mn} e^{2\pi i \nu_{mn} t}; \qquad p_{mn} = \bar{p}_{mn} e^{2\pi i \nu_{mn} t},$$

all of the matrix functions of them built up according to the foregoing idea will have associated with their mnth term the same time factor $e^{2\pi i \nu_{mn} t}$. This will turn out to be a very important property.

We define two matrices as equal to each other when the mnth term of one is equal to the mnth term of the other.

Now, a word with regard to what is called differentiation of one matrix with respect to another. Suppose that in ordinary algebra we have an expression z a function of x and y of, say, the form

$$z = x^2 + 3y + x^3 y.$$

We know what we mean by differentiating z partially with respect to x. We mean, make a small change in x while keeping y constant. Find the corresponding change in z. Proceed to the limit and so forth. But when we have said all this and found out how to carry out the process, we would differentiate the foregoing expression for z partially with respect to x by taking the 2 from the exponent of x^2 and putting it on the left hand side of x. We would rub out the $3y$, and we would take the 3 from the exponent of the x^3, put it at the left hand side and change the exponent of x^3 to 2. In this way we would obtain

$$\frac{\partial z}{\partial x} = 2x + 3x^2 y.$$

We would then interpret the result. Now this formal process of changing around the positions of the numbers is a perfectly

defined process irrespective of the more deep-seated meaning of differentiating. It would be a definite process even if there were no deep-seated meaning, although the chances are that it would then be of no great use. We can define differentiation as the procedure of carrying out the same operations with the symbols as we should carry out in ordinary differentiation. In other words, if we want to differentiate the matrix function

$$M = p^2 + q^2 + ap^2q^2 + bq^2p^2$$

partially with respect to p, we simply write

$$\frac{\partial M}{\partial p} = 2p + 0 + 2apq^2 + 2bq^2p$$

and this is what we call the partial differential coefficient concerned. All we have to be sure about is that it does represent a matrix, and of course it always must, by the very nature of its formation.

In matrix calculations we meet with such expressions for matrices as $1/(p^2 + q^2)$. Without further definition they would be meaningless. They become defined in a manner to give them meaning, however. Thus the quantity in question becomes defined as a matrix which when multiplied by the matrix $p^2 + q^2$ results in a matrix all of whose terms are zero, except those on a diagonal, these being unity.

I shall need only one more result in pure matrix theory, and this I shall quote without proof.

Suppose that φ is any matrix function of two other matrices p and q in the sense in which we have understood the word function. Then it can be shown that

$$\varphi q - q\varphi = \frac{h}{2\pi i}\frac{\partial \varphi}{\partial p}, \tag{20}$$

provided that the two matrices satisfy the condition that [4]

$$(pq - qp)_{mn} = h/2\pi i \qquad \text{for } m = n, \tag{21}$$

$$(pq - qp)_{mn} = 0 \qquad \text{for } m \neq n. \tag{22}$$

[4] The constant $h/2\pi i$ occurring in (20) and (21) could of course be replaced by any constant for the purpose of this theorem.

Here the differential coefficient $\partial\varphi/\partial p$ is understood in the sense of matrix differentiation as we have defined it.

Resumption of the Atomic Problem.—Let us return now to our problem of atomic structure. We think of a number of coördinates which we shall label as q_{mn}, and we want to determine from them some differential equations which will insure that they shall come out as functions of the time of the form

$$q_{mn} = \bar{q}_{mn} e^{2\pi i \nu_{mn} t}, \tag{23}$$

where ν_{mn} are of such a nature that they can all be determined in terms of a suitably assigned set of constants W for the atom by the relation

$$\nu_{mn} = \frac{W_m - W_n}{h}. \tag{24}$$

How can we bind ourselves in our differential equations so as to be sure that we shall secure this result?

Now if we adopt a sufficiently cold-blooded attitude towards the problem, we can solve it very easily. Thus, for example, suppose we take the Hamiltonian equations (1) and adopt for our Hamiltonian function the form

$$H = \frac{1}{2} \sum_m \sum_n \left\{ p_{mn}{}^2 + 4\pi^2 \left(\frac{W_m - W_n}{h} \right)^2 q_{mn}{}^2 \right\}, \tag{25}$$

where the different coördinates and momenta are to be obtained by attaching all integer values to m and n in q_{mn} and p_{mn}, and where the W's are suitable numbers characteristic of the atom. The Hamiltonian equations of course yield from the above value of H a harmonic function of the time for q_{mn}, the frequency being given by

$$\nu_{mn} = \frac{W_m - W_n}{h}.$$

Indeed, our expression for H is none other than that for a system of independent simple harmonic oscillators.

Thus, for example, if we should take

$$W_s = \frac{2\pi m_0 e^4}{h^3} \left(\frac{1}{s^2} \right),$$

the expression

$$\nu_{mn} = \frac{2\pi m_0 e^4}{h^3}\left(\frac{1}{m^2} - \frac{1}{n^2}\right)$$

resulting is that of the Balmer formula; and, by inserting an appropriate expression for the coefficient of q_{mn}^2 as a function of m and n in (25) it is obviously possible to obtain any form we may desire for ν_{mn}, such a form for example as that representing the relativity fine structure.

While such a procedure as that we have sketched could be made to give, formally, the frequencies associated with the atom, it would probably not be deemed satisfactory as a theory. For, quite apart from the dissociation of the procedure from any concrete picture of the mechanism, the necessity of specifying arbitrarily so many numbers (the W's) to fix our atom would rob the procedure of that element of simplicity of statement which characterizes what we mean by a theory. The features which determine whether a statement of atomic processes is to be designated as a theory or merely as an empirical collocation of facts is, in some measure a matter of convention. In so far as convention has crystallized itself, however, the custom has been to start with a mathematical form characteristic of the system (a Hamiltonian form, for example) and then, by means of differential equations, involving this form, express what we wish to express; but, it seems to be an unwritten requirement of the fundamental form that it shall contain as few constants characteristic of the atom as possible. Indeed, apart from fundamental constants like e and h, we have so far preferred to have no more than one constant characteristic of the atom. On Bohr's theory this constant is the atomic number.[5]

A very significant feature of the Bohr theory is its power to extract from the fundamental Hamiltonian form when taken in conjunction with the differential equations, and the quantum condition, all the values of W necessary to give

[5] We are here regarding the Hamiltonian function for a simple atom like hydrogen as obtained by specialization from the general Hamiltonian function for an atom with n electrons.

the frequencies, without the necessity of introducing these W's explicitly into the Hamiltonian form itself as was done in (25).

Of course the Bohr theory does not determine the frequencies as a dynamical consequence of the differential equations of motion. Indeed, the only type of differential equation which can lead to a simple harmonic motion of frequency ν_r for the coördinate q_r is the equation

$$\ddot{q}_r + 4\pi^2\nu_r^2 q_r = 0, \tag{26}$$

and to such equations the Bohr theory does not lead. The Hamiltonian equations can only lead to equations of this type when used in conjunction with a Hamiltonian function of the form

$$H = \tfrac{1}{2} \sum_r (p_r^2 + 4\pi^2\nu_r^2 q_r^2),$$

in which all the ν_r's are expressed in the Hamiltonian function H at the beginning; and it would appear that no system of ordinary differential equations even though they be different from the Hamiltonian equations could lead to differential equations of the type (26) without the explicit occurrence of the desired ν_r in the fundamental form figuring in the original differential equations. The equations with which we do have to deal usually go as far as having solutions which are periodic, simply or multiply, but not simple harmonic. When we analyze the periodic solution into harmonic terms we obtain Fourier series of such terms, and the relation between the frequencies in a Fourier series is fundamentally different from the relation between the frequencies in spectral lines.

If, however, the laws of our subject are expressed as relations between matrices, the fundamental difficulty as regards the frequencies disappears at one stroke. Thus, suppose that we consider a set of simple harmonic motions of the type $\overline{q}_{mn}e^{2\pi i\nu_{mn}t}$, and write them down in the form of a matrix, as we did above in the section under matrices. Suppose further that we consider any other set of quantities $\tilde{p}_{mn}e^{2\pi i\nu_{mn}t}$, forming a matrix with the same frequencies. We

may, if we like, consider any number of sets of such quantities forming matrices p, q, r, s, $\cdots$. If we differentiate any one of these matrices with regard to the time,[6] we obtain another matrix with the same frequencies; and the same conclusion holds no matter how many times we differentiate it. We have already seen that if we multiply any matrices such as p, q, etc., together, we get another matrix with the same frequencies so that, for example, in such a matrix equation as

$$\frac{d^2p}{dt^2} + a\left(\frac{dq}{dt}\right)^2 + bp^3q^2 = 0, \tag{27}$$

where a and b are constants, each of the matrices $\frac{d^2p}{dt^2}$, $a\frac{dq^2}{dt}$, bp^3q^2 would have as the coefficient of its mnth term the same time factor $e^{2\pi i\nu_{mn}t}$ as applied to the p matrix or the q matrix. Thus this time factor would cancel out in our equation and leave us with an equation involving only the amplitudes and frequencies, the latter having made their appearance in the time derivatives of the matrices. A similar remark applies irrespective of the degree of complexity of the differential equations of which (27) has been taken as an illustration. The net result of all this is that the procedure permits us to determine *one* frequency ν_{nm} as associated with any coördinate q_{nm}, without encumbering ourselves with a lot of other frequencies which we do not want. Had we sought any such result from an equation like (27) treated as a differential equation between ordinary quantities, we should find that it would not have been permitted. Thus, even for the case where there is only one p and one q, if we should substitute $q = \bar{q}e^{2\pi i\nu_{mn}t}$ and $p = \bar{p}e^{2\pi i\nu_{mn}t}$ in (27), while the first two terms of the equation would give a time factor $e^{2\pi i\nu_{mn}t}$, the second would give a time factor $e^{2\pi i(2\nu_{mn})t}$ and the third would give a time factor $e^{2\pi i(3\nu_{mn})t}$. Thus the time factor would not cancel out from the equation, and this is the way the equation has of telling us that it refuses to be satisfied by

[6] By this we mean, differentiate each term of the matrix with regard to the time.

any situation involving only one harmonic period for each coördinate. Looking back over our argument we see that it was the peculiar nature of matrix multiplication which led to the fact that functions of matrices were matrices with the same frequencies as those in the original matrix and so to the conclusion that the substitution of simple harmonic functions for the q_{mn}, etc., in the differential equations would result in the cancellation of the time factors from the equations, leaving equations to determine the ν_{mn} and incidentally the amplitudes.

If then we intend to broaden the basis for obtaining single simple harmonic frequencies for each coördinate beyond the stage provided for along classical lines in the use of the Hamiltonian function (25), we may do so by deciding to describe our laws in terms of the language of the matrix calculus, in the sense that all our equations shall be matrix equations. We may yet encounter difficulties in predicting the frequencies we desire, but we shall never encounter difficulty on the basis of requiring for each q_{mn} a simple harmonic frequency and nothing more.

If then we desire to have

$$\nu_{mn} = \frac{W_m - W_n}{h}, \tag{28}$$

we must have

$$2\pi i \nu_{mn} q_{mn} = \frac{2\pi i}{h}(W_m - W_n) q_{mn}. \tag{29}$$

If q_{mn} has to be of the form

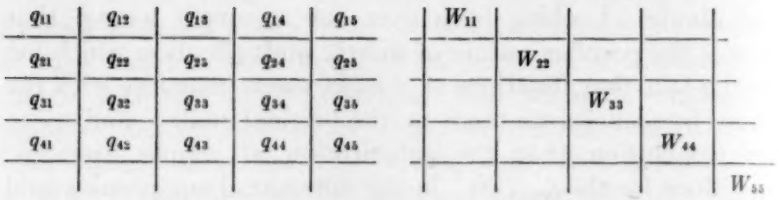
$$q_{mn} = \bar{q}_{mn} e^{2\pi i \nu_{mn} t}, \tag{30}$$

then the left-hand side of (29) must be $\dot{q}_{mn}$ so that we must have

$$\dot{q}_{mn} = \frac{2\pi i}{h}(W_m - W_n) q_{mn}. \tag{31}$$

Now let us write our q_{mn} in the form of a matrix.

FIG. 3.



In this we make no assumption, we simply construct a table. Let us prepare a chart for another matrix and relabel our W's, for convenience of notation, as $W_1 \equiv W_{11}$, $W_2 \equiv W_{22}$, $\cdots$. Let us insert them on this chart in the appropriate positions indicated by their subscripts. Here again, we assume nothing, we simply construct a chart. Now we may regard this W chart as a matrix with a lot of its elements zero. If we don't like this, we may write suitable W's in all the other spaces and then put them equal to zero whenever convenient. A matrix such as Fig. 3 is called a diagonal matrix.

Now it is a comparatively easy thing to prove that if we take the matrix product of the q matrix and the diagonal W matrix,

$$(Wq - qW)_{mn} = (W_m - W_n)q_{mn}. \tag{32}$$

Hence (31) becomes

$$\dot{q}_{mn} = \frac{2\pi i}{h}(Wq - qW)_{mn}, \tag{33}$$

where W and q are now matrices. So far the W's have no significance other than that of a set of numbers assignable to the atom. Now in the process of hunting in our mind for a specification of those features which were necessary to satisfy us in the formulation of a theory we realized that tradition had suggested that we avoid the arbitrary importation of numbers into our theories for the express purpose of fitting our needs and try to evolve them out of some mathematical form by the aid of suitable mathematical operations carried out on it. And so, in respect of our W's the thought has been to express them as the terms of a diagonal matrix H, which matrix is taken as a matrix function of the q matrix

and some other matrix, a p matrix having the same frequency terms associated with it. This process is analogous in the mathematical sense to the determination of the energy levels as certain numbers evolved from the Hamiltonian form chosen, by suitable operations thereon, these operations being expressed in the quantum conditions and in the Hamiltonian equations.

Now we recall that, in ordinary mechanics, the conditions *sufficient* in order that a certain function (the Hamiltonian function) of the p's and q's shall remain constant with time is that the p's and q's shall satisfy the Hamiltonian equations. A similar result holds in the matrix calculus. It can be shown that if H is a matrix function of two matrices p and q then the conditions

$$\dot{p}_{mn} = -\left(\frac{\partial H}{\partial q}\right)_{mn} \quad (34); \qquad \dot{q}_{mn} = \left(\frac{\partial H}{\partial p}\right)_{mn} \quad (35)$$

will insure that all the H_{mn} shall remain constant with the time. In other words they will insure that all the elements of the H matrix which depend on the time, i.e. all the elements other than the diagonal elements, will be zero. These constant values, which it is now the intention to associate with the numbers characteristic of the atom may, from this time onward be written either as H_{11}, H_{22}, $\cdots$, etc., or W_{11}, W_{22}, $\cdots$, etc. For a matrix of N columns and N rows [7] (34) and (35) would yield $2N^2$ equations which would serve to determine all the $\bar{p}_{mn}$ and $\bar{q}_{mn}$ in terms of the ν_{mn}, so that on substituting back in the H matrix, the constant values H_{11}, H_{22}, $\cdots$, etc., will involve only the ν_{mn}.

Now we have seen that in order that the frequencies may be of the form (28) it is necessary that (33), which we may now write as

$$\dot{q}_{mn} = \frac{2\pi i}{h}(Hq - qH)_{mn}, \quad (36)$$

[7] The matrices are of course infinite in extent. The extension to this case will probably cause no immediate trouble to the reader however, apart from conditions of convergency, etc., into which we will not now enter. However, a difficulty does arise in respect of the uniqueness of the solutions of the algebraic equations, and it is necessary to import additional criteria for the selection of the solutions to be accepted.

shall hold. And since the p_{mn}, by their very nature, must have the same frequencies as the q_{mn}, we must also have

$$\dot{p}_{mn} = \frac{2\pi i}{h}(Hp - pH)_{mn}. \tag{37}$$

Since we have already subjected our $\dot{q}_{mn}$ and $\dot{p}_{mn}$ to the condition (35), the satisfying of (36) and (37) requires that

$$(Hq - qH)_{mn} = \frac{h}{2\pi i}\left(\frac{\partial H}{\partial p}\right)_{mn}$$

and

$$(Hp - pH)_{mn} = -\frac{h}{2\pi i}\left(\frac{\partial H}{\partial q}\right)_{mn}.$$

But, as we have already stated in the section on matrices, these will be satisfied, and they will only be satisfied, if the p's and q's in addition to satisfying (34) and (35) also satisfy the conditions

$$(pq - qp)_{mn} = \frac{h}{2\pi i} \qquad \text{for } m = n, \tag{38}$$

$$(pq - qp)_{mn} = 0 \qquad \text{for } m \neq n. \tag{39}$$

These extra conditions supply N^2 additional equations for a matrix of N columns and N rows, so that in conjunction with (34) and (35) they serve to determine the ν_{mn}, the $\bar{q}_{mn}$ and the $\bar{p}_{mn}$, and so to leave nothing undetermined in the expressions for H_{11}, H_{22}, etc.[7] The numbers which ultimately characterize these quantities are numbers characteristic of the mathematical form of H and of the equations (34), (35), (38) and (39). The situation is loosely analogous to that of classical quantum theory at the stage where the application of the Hamiltonian equations to the Hamiltonian functions determines a series of energy levels depending on the initial conditions specified, while the addition of the quantum conditions here analogous to (38) and (39) removes those initial conditions from explicit expression and represents the energy levels entirely as functions of numbers inherent in the form of the Hamiltonian function and of the Hamiltonian equations, and quantum condition equations.

We may thus crystallize the whole situation as follows:

We may write down any matrix equations we like in terms of matrices p, q, $\cdots$, etc., and be certain that we can satisfy them by relations of the type

$$q_{mn} = \bar{q}_{mn} e^{2\pi i \nu_{mn} t} \quad (40); \qquad p_{mn} = \bar{p}_{mn} e^{2\pi i \nu_{mn} t}, \quad (41).$$

provided that

$$\nu_{mn} = \nu_{mk} + \nu_{kn},$$

so that instead of these types of solution being a characteristic of a very limited set of laws (differential equations), as in the older theories, they are solutions of *all* types of equations of the matrix form. The particular laws (34) and (35), however, serve to restrict the p's and q's to those which give for the values of the H_{mn} a set of numbers (constants independent of the time), one for each value of m and n, all the constants for the cases $m \neq n$ being zero. If, from all the possibilities for p's and q's admitted thus far, we restrict ourselves further to those satisfying the rather mysterious conditions (38) and (39), the ν_{mn} resulting become restricted to those satisfying (28), which is the analytical equivalent of the Bohr frequency condition. Indeed, (28) might be used in place of (38) and (39) with a resulting removal of the apparent element of artificiality inherent in these conditions.

When solutions of the type (40) and (41), with

$$\nu_{mn} = \nu_{mk} + \nu_{kn},$$

are substituted in (34), (35), (38), (39), the time factor cancels out in each equation as we have already remarked, and we are left with just enough equations to determine the ν_{mn}, the $\bar{p}_{mn}$ and the $\bar{q}_{mn}$. In terms of these we can also determine the H_{11}, H_{22}, etc., if we so wish. Not only is there no need of any extra conditions corresponding to the quantization of the integrals of pdq of classical quantum theory, but in so far as the amplitudes $\bar{p}_{mn}$ and $\bar{q}_{mn}$ provide criteria for determining the intensities and polarizations, there is no need of anything analogous to the correspondence principle. It will be observed, moreover, that since the

amplitudes are complex quantities, their determination serves to give the phases as well as the real amplitudes.

Extension to Many Degrees of Freedom.—In a calculus which deals with matrices and the like, degrees of freedom no longer have exactly what we may term the pictorial significance associated with them in ordinary dynamics. It is necessary for us to see in what sense the idea of "many degrees of freedom" played a part in the old quantum theory in order to see what must be done to secure corresponding features in the new theories. In the first place we have to provide for coördinates of different kinds, different in the sense necessary to provide for the phenomena of polarization. Then in the old quantum theory a system of one degree of freedom gave rise to an expression for the energy W which was a function of only one quantum number, in the sense that it could be written as a function of n, i.e. $W = W(n)$, with the understanding that the totality of W's available were to be obtained by assigning all integer values to n. The frequencies which resulted could be written in the form

$$h\nu(mn) = W(m) - W(n),$$

in which the notation is here to be taken as indicating that the frequency $\nu(mn)$ is to be obtained by assigning definite integers m and n in W and then taking the difference of the W's so obtained. We have purposely changed our notation slightly from that so far used, and have written $W(m)$ and $W(n)$ instead of W_m and W_n, which we formerly used for the purpose of *ordering* the W's without, however, implying that they were functions of a single set of integers in the sense implied above.

In the old quantum theory, a system of two degrees of freedom was characterized by two quantum conditions $\int p_1 dq_1 = n_1 h$ and $\int p_2 dq_2 = n_2 h$, and these lead to the W's being functions of two quantum numbers n_1 and n_2, so that W could be appropriately written as $W(n_1, n_2)$, with the understanding that the totality of the different values of W was to be obtained by assigning all integer values to n_1 and n_2

in $W(n_1, n_2)$. Here the frequency equation could be written

$$h\nu(m_1m_2, n_1n_2) = W(m_1, m_2) - W(n_1, n_2).$$

The older quantum theory, through the expression of its quantum conditions in terms of integers n_1, n_2, etc., provides a guarantee from the start that the W's finally obtained will come out as functions of integers.

Now in the matrix theory, as we have presented it so far, the diagonal property of the W's in the matrices insures that they shall be characterized by one set of integers in the sense that their positions in the matrix are entirely determined by the assignment of the number n in the subscripts W_{nn}. This is a very different thing from implying that an algebraical functional relationship exists between the W's and the number n, and it is of course an algebraical functional relationship which we must have if the various mathematical operations already assumed are to be permitted. It is, moreover, not inconceivable that the totality of W's of our simple two-dimensional matrices already studied could be included in an expression of the type $W(n_1, n_2)$, or $W(n_1, n_2, \cdots, n_n)$, with the understanding that the W's are to be obtained by assigning successively the totality of integers to the numbers $n_1, n_2, \cdots$, etc., which integers would, however, no longer have any simple connection with the integers involved in specifying the matrix. Again, it is by no means obvious that the totality of W's expressed by $W(n_1, n_2)$, for example, might not also be expressed in another functional form as a function of a single set of integers. We thus see that there is not as unique a meaning as might be supposed to the statement that the W's are functions of one set of integers, or of two sets of integers, etc. However, I must not belabor these matters, and must fall back upon that rather undignified avenue of escape from his mathematical difficulties which the physicist takes when, sensing much possibility of trouble, from then on he cleans his slate, remarking that they are matters for the pure mathematician to worry himself over.

Having, therefore, at least made our confession we can

rest content in the thought that if the quantities in our matrices can be expressed as functions of the numbers which label the matrices, then, for the matrices we have already discussed, the W's will come out as functions of a single set of integers.

A method of securing W's which are functions of two sets of integers, subject to postulates similar to those stated above, now becomes clear. We must construct, instead of a two-dimensional matrix, a 2 x 2-dimensional matrix. In other words we must consider a set of q's and p's characterized by subscripts in the manner $q_{m_1, m_2; n_1, n_2}$ with frequencies $\nu_{m_1, m_2; n_1, n_2}$. We then write a Hamiltonian function of q and p, say $H(q, p)$, and, proceeding along lines analogous to those already discussed for the simple case, arrive at the conclusion that $q_{m_1, m_2; n_1, n_2}$ shall be of the form

$$q_{m_1, m_2; n_1, n_2} = \bar{q}_{m_1, m_2; n_1, n_2} e^{2\pi i \nu m_1, m_2; n_1, n_2 t}$$

with

$$h\nu_{m_1, m_2; n_1, n_2} = H_{m_1, m_2}(qp) - H_{n_1, n_2}(qp),$$

provided that the Hamiltonian equations hold and provided that $(pq - qp)_{m_1, m_2; n_1, n_2} = 0$ unless $m_1 = m_2 = n_1 = n_2$, and equals $h/2\pi i$ if $m_1 = m_2 = n_1 = n_2$. There is no formal necessity to accompany the process with the introduction of two different kinds of coördinates and momenta. Analogy with the older quantum theory and the anticipation of the necessity of providing for the properties of different states of polarization tempts us, however, to attach to the system a number of degrees of freedom equal to the number of quantum numbers. The result, which we shall not develop in detail since it presents no features to cause difficulty, then takes the form that, if we start with a Hamiltonian form,

$$H = H(q_1, q_2, \cdots, q_n; p_1, p_2, \cdots, p_n).$$

Then, the Hamiltonian equations

$$\dot{p}_k(m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n) = -\left(\frac{\partial H}{\partial q_k}\right)_{(m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n)}$$

$$\dot{q}_k(m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n) = \left(\frac{\partial H}{\partial p_k}\right)_{(m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n)}$$

combined with the restriction that

$$(p_k q_l - q_l p_k)_{m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n} = h/2\pi i \qquad (42)$$

if $k = l$, $m_1 = m_2 = n_1 = n_2 =$ etc., and

$$(p_k q_l - q_l p_k)_{m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n} = 0 \qquad (43)$$

if this condition does hold, these conditions provide for all the elements of the H matrix being independent of the time, i.e., they provide for a diagonal H matrix and they provide for simple harmonic vibrations of the form

$$(q_k)_{m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n} = (\overline{q}_k)_{m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n} e^{2\pi i \nu_{m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n} t}$$

with frequencies given by

$$h\nu_{m_1, m_2, \cdots, m_n; n_1, n_2, \cdots, n_n} = H_{m_1, m_2, \cdots, m_n} - H_{n_1, n_2, \cdots, n_n}.$$

Moreover, the complete set of equations is sufficient to determine all the amplitudes, phases and frequencies.[7]

In the Hamiltonian function and in the relations (42) and (43) will be found that tying up of the q_x's with the q_y's and q_z's which will prevent all of these from acting entirely independently.

In the practical application of the theory everything hinges upon the appropriate choice of the matrix Hamiltonian function H. Once this is assigned the whole story of the atom is told. Now in choosing the Hamiltonian function the procedure seems to be to write for it the same matrix function of the p's and q's as would be chosen for the ordinary algebraical function of the p's and q's if one were setting out to attack the problem along the lines of the old Bohr theory. This seems a curious procedure. For the whole pictorial structure of electron orbits which suggested the Hamiltonian equations in the classical quantum theory has evaporated. The choice of the Hamiltonian function for the hydrogen

atom is determined on the old mechanics by the idea that the atom consists of a proton with an electron revolving around it. It is for this system that the Hamiltonian function is chosen. We obtain elliptic motions with the coördinates varying with the time in a manner determined by a whole train of Fourier frequencies. The choice of the corresponding function on the matrix theory is not expected to lead to time variations of the coördinates of anything like this type. It is simple harmonic motions related by the Ritz combination principle which are desired and provided for. On performing the matrix differentiations with respect to the coördinates the resulting matrix obtained from each of the Hamiltonian equations has the same form as the expression for the corresponding algebraical function of the Bohr theory. However, the process of picking out the mnth term of $(\partial H/\partial q)$, for example, in order to equate it to $-\dot{p}_m$, is, in general, an exceedingly difficult matter, and there is no reason to expect any similarity whatever between the equations which result and any of the equations of the classical theory. It is difficult to see why that mathematical form of a Hamiltonian function which, when subjected to the analysis of the Bohr theory, led to a certain spectral series should, when subjected to the entirely different treatment of the matrix theory, conserve in itself the story of that series. The technique of the operation of the conditions (42) and (43) is moreover quite different from that involved in the quantization of the integrals of pdq. If, in quantizing in terms of the old Bohr theory, we had been asked to multiply the angular momentum in one ellipse by the radial momentum in another ellipse, and after building up a lot of quantities in which the properties of all the orbits were mixed up, we were asked to quantize that, we should have a condition somewhat analogous to that demanded by the conditions (42) and (43) in the matrix theory. We were even alarmed when the correspondence principle asked us to determine intensities by talking about two orbits at once. However, when we have gotten over the shock of such a radical departure from the classical

quantum method, we have to realize that it is really a strong point in favor of the new theory that it wishes to speak of the whole atom at once. The attempt to speak of individual orbits and then discuss perturbations between them was the thing which led to a hopeless complexity, and indeed to ambiguity in the process of quantization in the classical quantum theory. Nature seems to have a way of rejuvenating its simplicity at certain stages of development in its complexity. A molecule of HCl becomes a terribly complicated thing when thought of in terms of individual orbits with quantization rules of the old Bohr theory. When thought of as a dumbbell, and very much the same old rules are again applied in this coarser view of macroscopicness, it again becomes relatively simple, as the theory of band spectra shows. One is almost tempted to think of a certain conservation of law form as we pass from one degree of macroscopicness to another. If then we have sufficient faith, there is something pleasing in the thought that nature may present us in each atom with a relatively simple problem for a type of an analysis which talks of the atom as a whole. It is at any rate an experimental fact that the behavior of even the most complicated atom seems to preserve a degree of regularity and simplicity which almost surprises one who thinks what he might expect on the Bohr theory even were he endowed with infinite mathematical skill to work out, properly, all the perturbations of what he might suppose to be the orbits in the atom, and then perform some marvelous transformation of coördinates in terms of which his quantization process would have meaning. It does seem a curious procedure for choosing a Hamiltonian function to take for it a form which was expressly designed as appropriate to the theory we have discarded. Yet, if we do not bind ourselves in some way in the choice of our Hamiltonian function, we run the danger of inviting a greater degree of empiricism than most people would stand for. When all is said and done, however, it does seem curious that, in seeking for a Hamiltonian function for our new theories, we should alight

upon the practice of engaging the old Hamiltonian function which used to serve in the Bohr theory, because it lost its job when that theory gave out.

To my own mind, the ideal procedure in applying such a theory as the matrix theory would be, if it were possible to carry out the plan, to start with the empirical expression for the series given by the classical spectroscopist, in a form of course embodying the Ritz principle, then having picked out from this the values of W_1, W_2, W_3, endeavor to construct a Hamiltonian function such that these were the diagonal elements.

By using the classical form for the energy of a dipole simple harmonic oscillator, Born has solved the problem of this oscillator. He has shown that it can only emit a single frequency equal to its classical frequency and that the values of W associated with it are $\frac{1}{2}h\nu_0$, $\frac{3}{2}h\nu_0$, $\cdots$. He has further found the expression for the amplitudes of the oscillations corresponding to the various single quantum jumps of the older theories, the frequencies corresponding to these single quantum jumps being the only ones which the theory permits to exist. Thus, the series of energy levels starting with $\frac{1}{2}h\nu_0$ and increasing by $h\nu_0$ seems to be provided for in a natural way as part of the theory, in contrast with the classical quantum theory which has no definite suggestion as to the $\frac{1}{2}h\nu_0$.

Here, however, I must pause for a few remarks. The matrix theory does not provide for radiation. It provides for a coördinate q_{mn}, for example, changing simple harmonically with the time, but it does not require that waves are emitted. Indeed, the story it tells is one in which all the H's remain constant with the time. The mathematical picture is not one of something having an energy H_1 and then jumping to a place where it has an energy H_2 leaving the atom with less energy than before, because of the emission of radiation of amount $H_1 - H_2$. The picture is that of a series of values of H each constant for all time, and a set of simple harmonic vibrations with frequencies $(H_m - H_n)/h$

going on continually for all time. One might attempt to avoid the difficulty by saying that the oscillators radiate no energy, but only a sort of ghost field of influence which determines what would happen elsewhere. We cannot dodge the difficulty so easily, however, for in attempting to do so we should, in the case of the production of photoelectric effect in an atom B as a result of radiation from an atom A, involve ourselves in the conclusion that the atom A could, without any change in its state, go on ejecting electrons from atoms like B for ever. Moreover, in a strict formal interpretation of the results, even admitting that radiation takes place, there is nothing to limit the individual amounts of it to h times the frequency emitted. Truly, the theory makes the frequency equal to that given by $h\nu = H_m - H_n$, but the coördinate can go on changing with this frequency for ever as far as the theory is concerned. Moreover, the conclusion with regard to the energy levels of the simple oscillator is not as satisfactory as appears at first sight. For the significance of those energy levels in their application to specific heats, for example, was this: On calculating by the usual formula of statistical mechanics, the probability that the oscillator was in any one of these states and then taking the average value of the energy in all these states an average value appropriate to the specific heat data was obtained. Here, however, the literal reading of the theory presents us with all of these energy states present at all time.

We may appear to avoid these difficulties by placing a different interpretation on the theory. We may say that the procedure is not to interpret the theory literally, but to go through the process of calculating the frequencies, amplitudes, and phases by it and then set up a sort of correspondence between the results of this calculation and nature by saying that the frequencies calculated represent the frequencies which may be emitted, and that when they are emitted there is a real change in the atomic energy corresponding to the difference between the W's associated with the frequencies actually emitted. The success of the pre-

dicted energy levels for the simple oscillator would then depend upon the importation of the usual probability factor of statistical mechanics to determine the number of the systems in any one of the energy states assigned. Unfortunately, on such a view we are left with uncertainty as to the meaning of the intensities of the lines in an atom in terms of the amplitudes calculated by the theory, since the measured intensity will depend not only upon the amplitude of the emitted vibration but also upon the average time during which the process is going on. In cases where the frequencies whose amplitudes are to be compared are all associated with changes in energy levels which are near together, as in the Zeeman effect, we might be justified in assuming with some doubtful plausibility that the relative intensities were proportional to the relative values of the squares of the amplitudes of the corresponding oscillators. Of course we might attach a formal significance to the amplitudes by supposing their squares to measure the relative probabilities of the corresponding transitions and then secure definiteness as regards the energy emitted by supposing that each transition is accompanied by an emission of energy equal to h times the corresponding frequency. Such an arbitrary procedure could hardly be expected to reflect the truth in the case of comparison of widely different frequencies. Moreover, in such a scheme of correlation, the last suspicion of our picture of oscillations vanishes.

Apart from these difficulties the theory has certain beautiful things to say. One of its most attractive achievements lies in its power to predict absence of certain lines. If the theory predicts that any of the q_{mn} are zero, then on almost any interpretation of the formal theory we should expect the corresponding frequency to be absent in the spectrum.

In its process of yielding the q_{mn} it may well be supposed that the theory will in many cases predict that some of them are zero. Thus suppose we write for a three-dimensional system

$$(M_x)_{mn} = (q_y p_z - p_y q_z)_{mn},$$
$$(M_y)_{mn} = (q_z p_x - p_z q_x)_{mn},$$
$$(M_z)_{mn} = (q_x p_y - p_x q_y)_{mn}.$$

It will be observed that the corresponding expressions on classical theory would be the three components of the angular momentum, although it is unnecessary for us to take any formal cognizance of this. It is possible to show that if our Hamiltonian function (which of course controls everything) leads to $dM_z/dt = 0$, then

$$q_{xmn}(M_{zmm} - M_{znn}) = \frac{h}{2\pi i} q_{ymn}, \tag{44}$$

$$q_{ymn}(M_{zmm} - M_{znn}) = -\frac{h}{2\pi i} q_{xmn}, \tag{45}$$

$$q_{zmn}(M_{zmm} - M_{znn}) = 0. \tag{46}$$

These tell us that if the Hamiltonian function is such as to lead to the conclusion that, for example, $M_{zmm} - M_{znn}$ is not zero, then there will be no amplitude for the mn frequency parallel to the axis of z. The plane of oscillation is perpendicular to the z axis. On the other hand, if $M_{zmm} - M_{znn} = 0$, then q_{ymn} and q_{xmn} are zero, and the vibrations take place parallel to the z axis.

Again, in the former case, it is possible to show from (44)–(46) that

$$\left[(M_{zmm} - M_{znn})^2 - \frac{h^2}{4\pi^2}\right] q_{xmn} = 0,$$
$$\left[(M_{zmm} - M_{znn})^2 - \frac{h^2}{4\pi^2}\right] q_{ymn} = 0,$$

and this tells us that if, for example, $M_{zmm} - M_{znn}$ is not zero so that, as we have seen, q_{zmn} is zero, then

$$M_{zmm} - M_{znn} = \pm\frac{h}{2\pi} \qquad \text{or} \qquad q_{xmn} = 0 \text{ and } q_{ymn} = 0.$$

In other words, only for those values of n and m for which the difference of the M's is $h/2\pi$ can we get any amplitude

perpendicular to z for the frequency ν_{mn}. And, as we have seen, only for the case where $M_{zmm} - M_{znn} = 0$ can we get any amplitude to the frequency ν_{mn} parallel to z. The net result of all this is that if the Hamiltonian function is such as to lead to $\dot{M}_z = 0$, then a very complete story of the absence of certain vibrations is obtained by calculating the M_x, M_y and M_z, and discussing the amplitudes and polarizations for the frequency ν_{nm} by considering the values of $M_{zmm} - M_{znn}$. Only where this quantity is zero or $\pm h/2$ do we get any amplitudes at all. If it is zero, the vibration is parallel to z; if $\pm h/2$, it is perpendicular to z. In the old Bohr theory a similar discussion was made in terms of the quantity M which there figured in a dynamical sense as the angular momentum of the orbit. In that theory it was only possible to make the discussion by way of the principle of correspondence. Here, everything comes out of the primary theory itself.

Pauli has attacked the problem of the hydrogen atom and has been successful in predicting the Balmer spectrum, but he has not calculated the relative intensities.

Of course, quite apart from the difficulties I have cited, such a theory as the Born and Heisenberg will appear quite incomprehensible to one who will think only in terms of our customary mathematical methods. The process of calculation from the original equations to the final results is not one which is carried out according to the ordinary fundamental laws of algebra. The process is perfectly well *defined*, however, and the theory of the atom is that we are to carry out that process in order to predict what will happen in the atom. If there be any who are so tied to the fundamental laws of algebra that they are unable to understand that such processes as those used in the Born and Heisenberg theory can have any significance, I can only say that the procedure is something like one where our theory of the atom is such that we can calculate the results required by setting up certain equations, proceed to develop them and then, when we have gotten to a certain stage, proceed intentionally to

make a number of mistakes in the mathematics, but in a perfectly systematic manner, so that anybody else doing the same kind of thing would know how to make the same mistakes. We are then to go on to the end. The theory is that the results we get in this way will agree with nature. The meaning of the process may be vague to some, but the process of manipulation of the symbols is perfectly definite and it is this which constitutes the theory.

THE THEORY OF SCHRÖDINGER.

No sooner had the Born and Heisenberg theory taken hold of the imagination than, only about a year ago, there appeared a paper by E. Schrödinger which seemed to find in an entirely different realm of mathematics a law structure which promised well to duplicate the law structure of the atom.

I have emphasized how in the case of the classical and in the matrix theory the story of the appropriate energy levels is determined by the form chosen for a certain expression, the Hamiltonian function, and the nature of certain mathematical operations performed upon it. Thus in the case of the hydrogen atom, the Hamiltonian function is

$$\frac{1}{2m}(p_x^2 + p_y^2 + p_z^2) - \frac{e^2}{r}.$$

It is a curious thing that while the nature of the operations performed on this function in the matrix theory seems so different from those performed in the classical theory, the values which the restricting conditions permit for it, the energy levels, come out the same on both theories. Now the vital thing in the Schrödinger theory is this: Illustrating the matter for a moment by the hydrogen atom, let us write down the differential equation

$$\nabla^2\psi + \frac{8\pi^2 m}{h^2}\left(E + \frac{e^2}{r}\right)\psi = 0.$$

Then, confining ourselves for the moment to $E < 0$, it is a mathematical fact that, quite irrespective of what ψ may mean, or what E may mean, only for certain values of the

constant E are there solutions for ψ which are finite, continuous, single valued and differentiable throughout space.[8] The values of E in question are essentially the same values as those which correspond to the quantized energy levels obtained on the Bohr theory or on the matrix theory. For the moment we need not concern ourselves as to how this result may have been guessed. It is sufficient that it is a mathematical fact. The only element in this expression which is common to the matrix theory and the Bohr theory is the quantity e^2/r. Even $\frac{1}{2m}(p_x^2 + p_y^2 + p_z^2)$ has vanished explicitly. We have found another way of doing things to e^2/r by which we may predict our energy levels or, since now there has been no mention of dynamics, we had better say another way of fixing certain constants associated with the form e^2/r. What a wonderful but silent story is contained in the form e^2/r! We have found at least three different ways in which we may bombard it with mathematical implements, and in all three it comes out of the ordeal calling out the numbers

$$E = -\frac{2\pi^2 me^4}{h^2}\left(\frac{1}{1^2}, \frac{1}{2^2}, \frac{1}{3^2}, \cdots\right).$$

May it not be that there is for every atom some V (in this case e^2/r), which on insertion in the equation

$$\nabla^2\psi + \frac{8\pi^2 m}{h^2}(E - V)\psi = 0$$

will cause that equation to give values of ψ which are finite, single valued, continuous and differentiable throughout all space only for certain values of E which turn out to be the appropriate so-called energy levels? What a curious thing if the V found necessary were always exactly the same old V which was used in classical dynamics of the Bohr theory for the corresponding atom.

[8] Any mystery attaching to this statement will vanish if we only realize that all it means is that in general the solution of the equation becomes infinite for $r = 0$ or $r = \infty$, but the part which becomes infinite has as a factor a function of $E + a_1$, $E + a_2$, etc., which vanishes for the critical values $-a_1$, $-a_2$, etc.

Now of course nobody likes to start off with a differential equation like this, which has dropped as it were from Heaven. One feels in a more dignified position if he has deduced it from something. It may be that the something may have no more claim to be true than the differential equation, but it is at least free from the stigma of being immediately useful; and, even as some people obtain a reputation for great profundity of thought by never saying anything, so this father expression by its very silence in an explicit sense is apt to pose as some more fundamental expression of hidden mysteries than is its chattering offspring, the differential equation, which appears to have descended to the low companionship of atoms and molecules. There are also certain very dignified mathematical principles in the field. The process of determining the condition that something shall be a minimum is a particularly dignified one. And, if you have an equation, and can show that it is a condition that something is a minimum, that elevates to importance both the equation and the thing which appears to want to be a minimum, even though nobody may have seen this latter animal before. His insistence on being a minimum gives him the entry into any scientific circles.

In his first attempt to give a meaning for the existence of this equation, Schrödinger starts out with the Hamiltonian function H for a conservative system, and writes down the equation

$$\frac{1}{2m}(p_x^2 + p_y^2 + p_z^2) - \frac{e^2}{r} = E. \qquad (47)$$

We confine ourselves to the case for a hydrogen atom in order to avoid too great abstractness in presentation. He then introduces a function ψ defined by

$$S = \kappa \log \psi,$$

where S is the action, so that

$$p_x = \frac{\partial S}{\partial x}, \qquad p_y = \frac{\partial S}{\partial y}, \qquad p_z = \frac{\partial S}{\partial z}.$$

Equation (47) then becomes

$$\left(\frac{\partial\psi}{\partial x}\right)^2+\left(\frac{\partial\psi}{\partial y}\right)^2+\left(\frac{\partial\psi}{\partial z}\right)^2-\frac{2m}{\kappa^2}\left(E+\frac{e^2}{r}\right)\psi^2=0. \quad (48)$$

He then proceeds to do a curious thing, viz. to seize upon the left-hand side of (48), and, without any cognizance of its being equal to zero, integrates it throughout all space, and determines the condition that it shall be a minimum. The condition is

$$\nabla^2\psi+\frac{2m}{\kappa^2}\left(E+\frac{e^2}{r}\right)\psi=0, \quad (49)$$

together with the requirement that $\partial\psi/\partial n$, the normal derivative of ψ, shall vanish to an appropriate order at an infinite boundary.

This, as a demonstration, would appear to be illogical. For the values of ψ which satisfy (49) do not in general satisfy (48); and the actual values of ψ found by Schrödinger certainly do not satisfy (48). Indeed there is no reason that they should, because (48) is by no means consistent with the minimization of the left-hand side of the equation throughout space. I only call attention to this point so that any one who reads Schrödinger's first paper will not be confronted with a stumbling block which will prevent his going on to the others. I do not suppose that Professor Schrödinger would support the first derivation at this time. Let me make quite clear that I am not claiming that (49) is wrong or that it is inconsistent with (47). It is certainly inconsistent with (48), but that only means inconsistency with (47) if $S=\kappa\log\psi$, and there is no need to assume this. It is only necessary to modify the *statement* of the procedure to avoid any inconsistency. The procedure is to take the Hamiltonian function and form the expression obtained when E is subtracted from it. We must then replace p_x by $\frac{1}{\psi}\left(\frac{\partial\psi}{\partial x}, \frac{\partial\psi}{\partial y}, \frac{\partial\psi}{\partial z}\right)$. This is simply a way of forming an expression, and by no means implies equality between the expression so formed and the one from which it was formed.

The starting assumption in the deduction of the Schrödinger equation may then be taken as the assumption that the volume integral of the expression so formed taken throughout all space has to be a minimum subject to ψ vanishing to a suitable order at infinity. It is essentially in this way that Schrödinger has represented his equation as the result of the application of a minimum principle in his later papers. In this way it appears as the result of a hypothesis which is independent of the Hamiltonian partial differential equation, in the sense that there is no implication that it is deduced from it.

In the Schrödinger equation itself there is nothing left of dynamics. The values of a certain constant E associated with the particular equation applicable to the atom under consideration are determined.[9] There must then be an extra hypothesis to the effect that the frequencies emitted by the atom are given by the differences of the possible values of E divided by h. In this form, and without anything further, the theory would have a valuable significance as a process of calculating the energy levels. It would not add anything to the Bohr theory in the matter of giving polarizations and intensities, however, and it would lack any pictorial representation of the phenomenon of the emission of the radiation such as the physical mind seems to crave in some form or other. In his second and later papers, however, while holding to the utilization of the differential equation (49), Schrödinger has built up a very beautiful mathematical structure which has in it that most desirable feature from the point of view of the physicist, the feature of speaking in terms of quantities which can be associated in the limiting cases with quantities with which we have become familiar in our former work, quantities such as charge density and the like.

A very beautiful guiding idea in Schrödinger's extension

[9] Of course, when the values of E given by the Schrödinger theory come out the same as those which would be given by the Bohr theory applied to the Hamiltonian function whose form is taken as a basis for suggesting the form of (49), it is always possible to permit ourselves the luxury of the Bohr atomic picture if we desire it.

of his theory in the picture of a wave mechanics is this: In optics, when speaking of phenomena concerned with large objects, we speak of rays of light, i.e. particles of light moving according to the principle of least time. If we try to explain diffraction in terms of these rays we fail, for this phenomenon is not included in the content of their possibilities, and it becomes necessary to introduce the wave idea. When we start to theorize upon the production of the waves, however, we again introduce some particles, the electrons in the atomic orbits, as responsible for the waves, and we have subjected these electrons to dynamical laws of motion just as we subjected the light particles to dynamical laws of motion. This might be all very well when the waves dealt with are very long, so that our particle is, let us say, a charged sphere oscillating up and down through an amplitude of a centimeter and producing a long electromagnetic wave. May it not be, however, that the idea of a particle, even as a center of the wave disturbance, loses its significance and has not enough content to explain the phenomena in the case of radiation emitted from an entity of the dimensions of an atom? The mind has had a very consistent desire to interpret nature's laws in terms of particles of some kind to which numbers may be attached for describing relations between them in such a way as to set up a correspondence between their behaviors and the phenomena of nature. May this not be the wrong procedure, however?

Let us trace the analogous history of our difficulty in the classical optical theory of rays and see how we got over it. The starting point of our picture is the wave surface, or rather a series of wave surfaces drawn about a point, a wave surface being the locus of all the points occupied at one time by a certain disturbance, a certain phase, emanating from the source. The path of the ray, of the light particle, from the center of disturbance to any other point through which a particle is known to pass is determined by the condition that the time taken along that path is less than for any other joining those points. In an isotropic but not necessarily

homogeneous medium, our about-to-be-discarded particle, the light particle, moves everywhere normal to the wave surface. All is well so long as we do not place an obstacle in the path of the light. But if we do insert such an obstacle, the construction for the rays leads to a sharp boundary separating the regions where there is light from those where there is no light. It can never indicate the diffraction bands which experiments reveal. What do we do about the matter? In spite of our use of the term wave surface, there has been as yet no introduction of the idea of a periodic motion. The true wave, with its concept of frequency and wave-length, has not yet made its appearance. We introduce it either by the crude picture of the construction of Huygens wavelets, or in the more complete manner by the introduction of the wave equation, which, in the case of an isotropic medium, is

$$\nabla^2\varphi + \frac{1}{u^2}\frac{\partial^2\varphi}{\partial t^2} = 0,$$

where u is that very phase velocity which we used in connection with our wave surfaces and is a quantity varying from point to point in the case of a non-homogeneous medium. Diffraction as regards the quantity φ then follows directly we introduce the mathematical specification (boundary conditions) appropriate to our obstacle. We think no more of our particle. Our light intensity is expressed by something proportional to the intensity (square of amplitude) of our quantity φ and not by the number of light particles passing one square centimeter per second. Provided we do not return to too small scaled phenomena, however, involving diffraction and the like, we can always recall our particle to represent in its own way the flux of wave energy. Let us now trace the analogies of this procedure applicable to the point of view that all particle dynamics has representation in nature only in the same sense that light corpuscles crudely represent rays of light.

We start off with our old dynamical ideas expressed by

$$\dot{p}_r = -\frac{\partial H}{\partial q_r}; \qquad \dot{q}_r = \frac{\partial H}{\partial p_r},$$

where, for a single particle,

$$H = \frac{1}{2m}(p_x^2 + p_y^2 + p_z^2) + V(x, y, z).$$

On writing

$$p_x = \frac{\partial S}{\partial x}, \quad p_y = \frac{\partial S}{\partial y}, \quad p_z = \frac{\partial S}{\partial z},$$

the equations of motion insure that S satisfies our old friend the Hamilton-Jacobi equation

$$\frac{\partial S}{\partial t} + \frac{1}{2m}\left[\left(\frac{\partial S}{\partial x}\right)^2 + \left(\frac{\partial S}{\partial y}\right)^2 + \left(\frac{\partial S}{\partial z}\right)^2 + 2mV_{(x,y,z)}\right] = 0.$$

They also insure that if the system is conservative,

$$\frac{\partial S}{\partial t} = E,$$

where E is a constant for the dynamical path and is in fact the total energy.

If we put

$$S = -Et + f(x \cdot y \cdot z), \tag{51}$$

it is possible to obtain a solution for $f(x \cdot y \cdot z)$ by substituting in the Hamilton-Jacobi equation. There will be three arbitrary constants, only two of which will be ultimately arbitrary, since the determination of the p's in terms of S and their subsequent insertion in the expression for H must give for that quantity the value E already assigned. Strictly speaking, the value of S given by (51) is supposed to apply to the point x, y, z, occupied by the particle at the time t. However, the right-hand side of (51), considered simply as a mathematical function of x, y, z and t, has a perfectly definite value at all points of space at any one t. At any value of t, we can imagine a surface of constant S. Only one point of this surface is occupied by the particle under consideration; but all of the other points of this surface have the physical significance that they represent the positions at the time t of other particles which have the same energy E and which, at previous times, lay on some surface of constant S. If the surfaces of constant action form closed surfaces one inside

the other, we can regard one of them as converging upon a point. If this point be regarded as the simultaneous origin of particles ejected with constant energy in various directions, all of these particles will at any subsequent instant be on one of the surface S = constant specified by (51). As time progresses, the surfaces of constant action will move through the space according to equation (51) in the sense that if at any one instant there is a surface of points over which the action has the constant value S_0, then, at an instant dt later, it will be on another surface that we shall find the constant value S_0.

In fact, quite generally

$$dS = \text{grad } S \cdot dn + \frac{\partial S}{\partial t} dt,$$

where dn is the element of length perpendicular to S = const. If we interest ourselves in such values of dn and dt that dS is zero, we shall have

$$\frac{dn}{dt} = -\frac{\partial S}{\partial t} \Big/ \text{grad } S = \frac{E}{[2m(E - V)]^{1/2}}.$$

Thus the velocity u with which we shall have to move normal to our S surface at any instant in order always to accompany the same value of S is [10]

$$u = \frac{E}{[2m(E - V)]^{1/2}}.$$

You see our origin of particles is very analogous to a source of light in the optical problem. The particles themselves are very analogous to the light rays or light particles. The S surfaces are very analogous to the wave surfaces. Now in the optical problem, the transition from geometrical optics to a wave theory is made by throwing away the rays or particles, retaining only the velocity idea suggested by the wave surface, and then setting up a wave equation for a quantity φ, with the velocity u associated with the old

[10] Of course the velocity of a particle on the surface S = constant is not equal to the velocity of the surface as above defined.

wave surface inserted in that equation. The only thing the new theory has to look after in the matter of these discarded entities is that it shall, in the case of short wave-lengths, lead to a cancellation of φ for all regions where geometrical optics would give geometrical shadows. So in the present case Schrödinger throws away the particles, the electrons, retains only the velocity u, and introduces a new quantity ψ' obeying a wave-equation

$$\nabla^2\psi' - \frac{1}{u^2}\frac{\partial^2\psi'}{\partial t^2} = 0, \tag{52}$$

with

$$u^2 = \frac{E^2}{2m(E - V)}.$$

He then has to see to it that for sufficiently small wave-lengths it shall turn out that ψ' will have no appreciable values outside of the region of the paths of the old discarded electrons as calculated on the old classical theory. I must hasten to call attention to the fact that the waves associated with this equation have nothing whatever to do with the light waves as provided for by Schrödinger's theory. In fact there are two kinds of waves in this theory, as will appear later.

Now as regards the solutions of (52), Schrödinger supposes that with each E there is associated only one simple harmonic time variation of frequency ν given by $h\nu = E$.

Putting then

$$\psi' = \psi(x, y, z)e^{\pm 2\pi i(E/h)t},$$

so that

$$\ddot{\psi}' = -\frac{4\pi^2E^2}{h^2}\psi',$$

we derive

$$\nabla^2\psi + \frac{2m(E - V)}{E^2}\cdot\frac{4\pi^2E^2}{h^2}\psi = 0,$$

$$\nabla^2\psi + 8\pi^2m\frac{(E - V)}{h^2}\psi = 0, \tag{53}$$

which is the equation we spoke of at the outset when starting

to talk of Schrödinger's theory. The condition that this equation shall have solutions which are finite, single valued, continuous and differentiable through all space leads, as we have already remarked, to a limitation of the E's to certain possible values. In the case of the hydrogen atom, for which $V = -e^2/r$, for example, we have a solution for every value of $E > 0$, these solutions corresponding to what would be called the hyperbolic paths on the old Bohr theory. For E negative, however, there are only solutions for such values of E as are obtained by attaching integer values to n in

$$E = -\frac{2\pi^2 me^4}{h^2 n^2}.$$

With each value of n there are associated a definite number of solutions, just as in the classical quantum theory of the degenerate hydrogen atom there are associated with each quantum number a definite number of energy levels with the same E.

The next step is to define in terms of ψ' a quantity which it is appropriate to associate with charge density. The procedure is to write down a quantity Ω which is a sum of all the possible solutions of ψ' for all the possible E's, each solution being multiplied by an arbitrary real constant C,

$$\Omega = C_1 u_1 e^{(2\pi i E_1 t/h)+i\theta_1} + \cdots C_r u_r e^{(2\pi i E_r t/h)+i\theta_r}.$$

The u_r are solutions of the Schrödinger equation for the appropriate values of the E's.

Now let us write down the conjugate of this expression

$$\bar{\Omega} = C_1 u_1 e^{(-2\pi i E_1 t/h)-i\theta_1} \cdots C_r u_r e^{(-2\pi i E_r t/h)-i\theta_r},$$

and let us multiply Ω by its conjugate. We obtain

$$\Omega\bar{\Omega} = \sum_m \sum_n C_m C_n u_m u_n \cos\left[2\pi(E_m - E_n)\frac{t}{h} + \theta_m - \theta_n\right]. \quad (54)$$

Here then is something which has in it all the frequencies to be obtained by dividing the differences of the various possible values of E by h. It is something which, if only it can be

allowed to act as a source of electromagnetic vibrations, will give the sort of frequencies we want. The origin from which x, y, z are measured in the functions u is of course the origin from which V is measured in equation (53). Theoretically, $\Omega\bar{\Omega}$ is distributed throughout the whole of space. In practice, however, the function dies off very rapidly outside a very limited region.

This function $\Omega\bar{\Omega}$ is called the density of charge. It is multiplied at each point by, let us say, z, the z component of the distance of the point in question from the origin, and the result is integrated throughout all space, theoretically, although in practice throughout a very little of it, and is called the electric moment parallel to z. We see that this quantity changes with time in a manner determined by a number of harmonic frequencies equal to those appropriate to the atom. If we now superpose upon all this the electromagnetic equations, which serve to define, in terms of this moment, electric and magnetic vectors having the properties of the vectors of that theory, we complete a certain stage of our theory. The light waves are the waves of the electromagnetic vectors, not of the quantity ψ', whose waves, indeed, travel with variable velocity.

It is well to pause for a moment in order to inquire whether this importation of the electromagnetic theory can take place quite as easily as I have implied. It is quite true that if the density ρ, and the density times velocity ρv, be assigned in ordinary electromagnetic theory for all space and time, the fields E and H are uniquely determined in so far as they depend upon ρ and ρv. In the most logical form of the electromagnetic theory, the equations themselves form merely definitions of the fields E and H in terms of ρ and ρv. The form of the equations does, however, require one thing, viz. that ρ and ρv shall be bound together by the equation of continuity

$$\frac{\partial \rho}{\partial t} + \text{div } \rho v = 0. \tag{55}$$

Happily the functions u possess the property that $u_m u_n$ when

integrated throughout space gives a zero result. Were this not the case, the time factor in (54) would result in a lack of conservation of the total charge. We must suppose assigned to the density ρ at each point a velocity v which must be assigned so as to satisfy (55). This can always be done. For the purpose of calculating the electromagnetic vectors, however, at any rate to a first approximation at great distances from the charge distribution, only the time variation of the total moment calculated as above is necessary.

We observe that the intensity of the z components of any line should be proportional to the square of the corresponding integral,

$$[\int\int\int C_m C_n z(u_m u_n) dx dy dz]^2,$$

and each of the u_m and u_n is determined by the solution of (53) as a function of E_m and E_n respectively, subject to the conditions of finiteness, single valuedness, etc., already imposed. Now it has been shown, subject, however to certain additional assumptions that the triple integral is exactly equal to what on Heisenberg's theory would be the element of matrix q_{mn}, provided that the C's are chosen so as to normalize the functions Cu and moreover it has been shown that the frequencies predicted by the Schrödinger method are the same as those predicted by the Born and Heisenberg method. We may, in fact, regard the association of $\Omega\bar{\Omega}$ with electric density as the association necessary to secure conformity with the Born and Heisenberg theory.

Of course, there is much that remains obscure even on the Schrödinger theory. The mystery of the appropriate choice of the Hamiltonian function as a basis for suggesting the form of ψ is not as great as in the Born and Heisenberg theory, since, for large scale phenomena, the Schrödinger theory provides for a merging of wave groups into the paths of particles as calculated from dynamics with the same Hamiltonian function. In its literal interpretation, however, we do have associated with the atom for all time all the values of E's permitted by the solution, and the oscillation of

our charge doublet goes on forever. There is no provision for emission in quanta. As in the Born and Heisenberg theory we cannot escape the difficulty by christening the electromagnetic field as a ghost field which does not really carry energy. For the thing to be accounted for is how, in the case of the photoelectric effect, for example, one atom can do something to another without itself experiencing change.

The only escape from these uncertainties lies in the device cited in connection with the matrix theory—the device which operates through transition probabilities instead of continuous radiation. Unfortunately, such a procedure destroys the physical picture promised by the theory.[11]

Again, a mystery is presented by the problem of the Compton effect, and indeed of all those phenomena such as ionization potentials and the like, where energy, now in the form of a characteristic constant for which a differential equation has a solution, has to find a way of getting over into the ordinary $\frac{1}{2}mv^2$ form of laboratory experience; although the matter is perhaps not so vague of solution on the Schrödinger theory as on the matrix theory. Indeed most of these matters go back to what was, in electrodynamics, the problem of the force equation, the problem of how electricity behaves under the influence of the field in which it finds itself, and which here appears in the form: how does the function $\Omega\bar{\Omega}$ behave when an electromagnetic field from another atom comes upon it? Presumably the Schrödinger theory itself contains the spirit of an answer to these problems. For it would appear that the logical pursuit of the method to the inquiry as to what happens to an atom B when electromagnetic radiation visits it from an atom A is to set up for B a new Hamiltonian function[12] modified in some way by the presence of the electromagnetic field (in the theory of the Stark effect where the field is steady this is indeed the method

[11] For a possible remoulding of the theory in a manner designed to minimize these difficulties, see "Concepts in Quantum Theory" by W. F. G. Swann. To be published in the April issue of the Journal of the Institute.

[12] Since this address was delivered theories of the Compton effect and of the photoelectric effect have appeared.

adopted), then let the atom tell its own story in terms of the differential equation (52) and the subsequent procedure already traced. Presumably photoelectric effect will make its appearance by the appropriate solution for ψ' and so for $\Omega\bar{\Omega}$, being such that the density as defined in the Schrödinger manner will be found to confine itself more or less to a line which ultimately leaves the atom entirely, the region on this line at which the density is finite progressing along the line with a speed which an older interpretation of the photoelectric effect would have termed the velocity of the electron. In this or in any theory which provides for a continual radiation in all directions we are, however, confronted with a very serious difficulty when we seek the action of that field on neighboring atoms. For the experimental indications are that sometimes something happens, and sometimes it does not. The field does not provide for a certainty of electron emission, for example, but only for a probability of such emission. Another difficulty to be faced by the Schrödinger theory concerns the occurrence of charges in definite units—protons and electrons. The older theories could afford to leave this as axiomatic; but a theory which goes so far as to define the significance of charge density in the sense in which the Schrödinger theory defines it more or less binds itself to continue the story to its logical conclusion. It must provide for the fact that when that complicated distribution of the $\Omega\bar{\Omega}$ function which represents the charges within an atom unwinds a part of itself in providing for the exit of what we call an electron, it does so in such a way that the electron always has the same charge.

In bringing to a conclusion what I fear must have seemed to you a very long address, and in order to save myself from assassination by either of the opposite extremes of thought in atomic physics, I may perhaps remark that it has not been my intention to present a brief for or against the theories I have been speaking of. My rôle has been that of an expositor whose duty has been to give as much of an idea as he can of these theories in a short time, remoulding the lines

of attack occasionally, in order to arrive at what I have supposed to be their salient features in the shortest way. I have endeavored to point out a few of the fundamental differences between and similarities between these theories and the classical theories. While, as I have said, I hold no brief for or against these theories themselves, I will not disown a brief for the principle that, in seeking a correlation of the phenomena of the atom, we shall have to widen our horizon of understanding so as to admit the expression of laws in forms other than those to which we have been accustomed in the days which have passed. The question of what criteria are necessary in order that a theory shall appear satisfactory to us is a subtle one; but it is one which we must settle with ourselves. For we may rest assured that we shall never understand until we have first defined to ourselves the sense in which understanding is to have meaning to us. If we can completely emancipate ourselves from the slavedom of models, we may come to believe that a good theory is one in which, by the postulation of a few things, we deduce many; and we shall not worry ourselves too greatly in a search for some more fundamental significance to the postulates than the postulates themselves; for we shall recognize those illusory fundamentals of fundamentals, our age-long tormentors, as the Will o' the Wisps of Science.

A FORMULA FOR CALCULATING THE BOILING-POINTS OF NAPHTHALENE CORRESPONDING TO PRESSURES BETWEEN 824 MILLIMETERS AND 704 MILLIMETERS OF MERCURY: AND THE BOILING-POINT OF NAPHTHALENE AT NORMAL PRESSURE.*

BY

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THE usefulness of the boiling-point of naphthalene as a fixed-point in thermometry depends, to a great extent, upon the accuracy with which the variation of its boiling-point with pressure is known, and the ease with which the temperature corresponding to a pressure can be calculated.

Data connecting the boiling-point of naphthalene with pressure have been presented by Crafts [1] and by Jaquerod and Wassmer.[2]

Griffiths [3] attributes to Crafts [1] the following formula:

$$t = 218.06 + \frac{H - 760}{17.1} \quad \text{or} \quad t = 218.06 + 0.058(H - 760).$$

This is identical with that of Waidner and Burgess below, but for the value of the normal boiling temperature.

The equation given by Travers and Gwyer [4]

$$T_p = 217.68^\circ + 0.057(p - 760)$$

is based upon the data of Jaquerod and Wassmer.[2]

The expression of Holborn and Henning,[5]

$$t = t_{760} + 0.0585(p - 760) - 0.000025(p - 760)^2,$$

is derived from the figures of Crafts.[1]

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* Contribution from the Eppley Research Laboratory.

[1] *Bull. Soc. Chim. de Paris*, **39**, 277–289 (1883).

[2] *J. de Chim. Phys.*, **2**, 52–78 (1904).

[3] *Phil Trans.*, A, **182**, 43–72 (1890).

[4] *Proceed. Royal Soc.*, **74**, 528–538 (1905).

[5] *Ann. d Phys* **26**, 833–883 (1908).

Waidner and Burgess [6] deduce a formula "satisfying the data both of Crafts and of Jaquerod and Wassmer." It is

$$T_{760} = T_{obs.} - 0.058(H - 760).$$

Waidner and Burgess [7] question the accuracy of the determinations of Jaquerod and Wassmer upon the basis of the small volume of the bulb of their gas-thermometer, and its lack of shielding against the cooling-effect of condensation and radiation.

Crafts does not state in the article quoted by Holborn and Henning and by Waidner and Burgess by what means his data were acquired. However, in an article published in 1913, he speaks of having made "a table of vapor-pressures by aid of mercury thermometers." [8] Since the paper published by Crafts in 1883, containing the data made use of by the authors above, deals exclusively with the calibration of mercury-in-glass thermometers, it is probable that it is to these (mercury-in-glass) data that his 1913 paper refers. It would appear, therefore, that all of the formulæ listed above are derived from observations that may be open to question, if not to doubt.

In his 1913 paper,[8] Crafts gives the following equation expressing pressure in terms of temperature:

$$P = 62.816 + 2.0509(T - 130) + 0.038169(T - 130)^2 + 0.0002122(T - 130)^3 + 0.000001264(T - 130)^4.$$

This expression is derived from a series of observations with the constant-volume nitrogen thermometer.

He gives also a table of even values of temperature and the corresponding pressures interpolated by means of this equation. A calculation by the present author shows these figures from 214° to 222° (693.10 mm. to 830.34 mm.) to conform to a simple logarithmic formula of the type

$$\log t = n \log p + \log a$$

[6] *Bur. Stan. Bull.*, 7, 1, 4 (1910).
[7] Ibid., p. 5.
[8] *J. Chim. Phys.*, 11, 429–477 (1913).

with a maximum deviation of .019° and an average deviation of ± .002°. (Crafts' value for the boiling-point under normal pressure obtained from the mean of 28 groups of measurements with the constant-volume nitrogen thermometer (fifty separate measurements) is 218.06°. This agrees fairly well with the values obtained by Waidner and Burgess [9] for Kahlbaum's naphthalene, but not so well with those for this compound "once recrystallized.")

The Bureau of Standards, under "Other Standard Temperatures" [10] in a circular issued in 1919, connects the boiling-point of naphthalene with pressure by the relationship

$$C° = 217.96 + 0.058(p - 760).$$

The Reichsanstalt has established the naphthalene boiling-point as a secondary fixed-point by the following statement: "Along with the preceding fixed-points, by which the legal temperature scale is established, the following secondary fixed-points in addition can serve for measurements:

* * * * * * * * * * * * * * * *

"Boiling-point of naphthalene, $217.9_6° + 0.058(p - 760)$ for pressures between $p = 750$ and 760 mm." [11]

* * * * * * * * * * * * * * * *

Finck and Wilhelm [12] have published the results of a determination with the platinum resistance-thermometer of the boiling-points of naphthalene at a number of pressures between 799.44 millimeters and 699.26 millimeters. The work was done at the Bureau of Standards where the equipment for resistance thermometry is of the highest type. The naphthalene was that supplied by the Bureau for the calibration of bomb-calorimeters. It "was purified by two crystallizations from alcohol followed by sublimation in a vacuum." [13]

Their results they express by the equation:

$$t_p = 217.95 + 0.2075(t_p + 273.1) \log (p/760).$$

[9] *Bur. Stand. Bull.*, 7, 1, 2 (1910).
[10] *Bur. Stand. Circ.*, 35, 2 (1919).
[11] *Ann. d. Phys.*, 4, 75, 855 (1924).
[12] *J. Am. Chem. Soc.*, 47, 1577 (1925).
[13] *Bur. Stand. Circ.*, 25, 11 (1923).

The value, 217.95°, obtained by these investigators for the normal boiling-temperature of naphthalene is not far from that of the Bureau of Standards and that of the Reichsanstalt, and also that of 217.98° ± 0.01, obtained by Waidner and Burgess [14] for Kahlbaum's naphthalene "once recrystallized."

In addition to fitting the formula immediately above, the data of Finck and Wilhelm also fit with equal closeness a formula which is identical in form with that of the one presented below in this paper, and the constants of which are very nearly in agreement. However, the exact values of these constants are not of importance to this research.

In order further to test the applicability of the logarithmic type of formula, a determination of the boiling-points of naphthalene at a number of pressures above and below normal pressure was undertaken in this Laboratory.

The naphthalene used was that supplied by the Bureau of Standards for the calibration of bomb calorimeters.[15]

Boiling was carried out under the conditions set forth in the "Standardization of the Sulphur Boiling Point," [16] the heater being an electric one. Pressure was regulated by means of an assemblage [17] in all essentials identical with that therein described, except that a "meter-prover with water seal" was omitted in the majority of readings. (In certain readings, as noted, a similar gasometer arrangement was attached, but no advantage due to its use was apparent.) The different pressures desired were secured by exhausting the air in the "trap," which had a capacity of some eight liters, or by pumping up pressure in it. The pressure remained sufficiently constant for the required readings to be made readily.

Pressure-differences were read on the water-manometer by means of a cathetometer accurate to about .05 millimeter, such an instrument being available and convenient. Readings

[14] *Bur. Stand. Bull.*, **7**, 1, 2 (1910).
[15] *Bur. Stand. Circ.*, **25**, 11 (1923).
[16] *Bur. Stand. Sci. Paper*, **339**, 183–184.
[17] Ibid., p. 180.

were reduced to millimeters of mercury at 0° C., and sea-level and 45° latitude. No corrections for difference in height of water-meniscus and barometer-cistern were made, a calculation having shown the errors due to this to be within the experimental error.

Atmospheric pressures were given either by a Fuess barometer, graduated to 0.02 millimeter, which had been compared with a Fortin type instrument certified at the National Physical Laboratory, Teddington, England, or by this latter itself. (It could be read to 0.05 millimeter.) The Fortin barometer was encased so as to reduce, as much as could be, any difference between the temperature indicated by the attached thermometer and that of the mercury. All readings were reduced to 0° C. and sea-level and 45° latitude.

(In connection with the measurement of barometric pressures, it should be noted that the "precision" of the instrument may give rise to an appearance of accuracy that is misleading. Chappuis [18] compared a Fuess barometer with his "normal barometer" and found a maximum deviation of 0.06 millimeter, and a mean deviation of 0.031 millimeter, the probable error being 0.03 millimeter. Day and Sosman [19] claim their two Fuess barometers to be "correct in their absolute value within 0.05 mm." In a private communication descriptive of a test made upon another Fortin type barometer from this Laboratory, the Bureau of Standards mentions irregularities, brought to light by the intercomparison of a number of barometers at the same time, which would indicate that ± 0.06 millimeter was the accuracy to be expected with the best laboratory instruments commonly available for pressure determinations.)

The thermometer-bridge, made by the Leeds and Northrup Company, was of the type designed by Mueller,[20] but was immersed in oil to a depth such that the dial-switches and all other metal fittings in the bridge-circuit were well covered.

[18] P. Chappuis, Extr. du Tome XVI. Travaux Memoires Bur. Int. des Poids, Mesures (1914), p. 14.

[19] Day and Sosman, *Carnegie Inst. Wash.*, **157**, 22 (1911).

[20] Mueller, *Bur. Stan. Sci. Paper*, **288**, 547 (1916).

The surface of the oil was protected from the effects of air-currents by a false top. The dial-switches were operated by handles at the ends of vertical upward extensions of the usual switch-shafts, or axes. These handles extended above the false top or cover in such a manner that no breach in the top resulted, and yet the cover could be raised or lowered as is any hinged lid, without supplementary manipulation. The resistance-coils of the higher values were enclosed.[21] The lowest dial gave steps of 0.0001 ohm, corresponding to two and a half to three divisions of galvanometer deflection per change of one "stud," with 0.5 milliampere through the bridge. (The measuring-current used was from 0.5 to 0.3 milliampere.)

The oil was maintained at 25° ± .05° by means of a thermostatic device which operated an electric heating-coil within the oil. Stirring was performed by an electric motor. The whole system was shielded as recommended by White,[22] and, in addition, was grounded. (It was found that it was essential to connect the case of the stirring-motor into the shield.)

The usual type of commutator [20] for reversing thermometer leads was used.

The bridge was calibrated [23] at the beginning of the determinations and at their end, the interval between the two calibrations being about one year. All corrections were extremely small, the largest, those on the ten ohm series of coils, being at the worst well within 0.01 per cent. All observations of resistance were corrected before the calculation of temperature.

The three thermometers used in this research were of the potential-terminal, "strain-free" design described by Sligh.[24] They were made in this Laboratory, with pyrex sheaths. When complete, they were annealed by frequent heating to

[21] Waidner, etc., *Bur. Stan. Sci. Paper*, **241**, 575 (1915).

[22] White, *J. Am. Chem. Soc.*, **36**, 2011 (1914).

[23] E. Griffiths, "Methods Measuring Temp.," C. Griffin & Co., pp. 49–51 (1918).

[24] T. S. Sligh, Jr., *Bur. Stan. Sci. Paper*, **407**, 53 (1921).

dull redness in a wire-wound tube-furnace, and cooling to room temperature. They were then calibrated. Thermometer E.L.-3 was chosen for the temperature-pressure work since its leads were so closely adjusted that practically no change in bridge-setting was required to restore a balance when the commutator was reversed.

The ice for the ice-point was frozen in block-tin from distilled water, in an ice machine. Conductivity determinations showed the water from the melted ice to be uncontaminated.[25] A Dewar flask served as container for the finely ground ice moistened with distilled water.

Water boiling-point resistances were determined in a hypsometer in all essentials identical with that described by Mueller and Sligh.[26]

Sulphur boiling-point resistances were obtained as prescribed,[27] using sulphur very kindly furnished by the Bureau of Standards from its own supply selected for this purpose The temperature corresponding to the reduced atmospheric pressure was calculated by the formula in the International Critical Tables.[28] The shields used were of sheet-iron, as specified.[29]

Platinum temperatures were converted to centigrade temperatures by means of a table of even values of "t," differing by 0.1°, and the corresponding values of "pt," calculated for each value of δ encountered, by means of the classical Callendar equation.[30] A calculating machine was used for mathematical work.

Table I contains the characteristics of the thermometers. While the δ value of thermometer E.L.-3 was not redetermined just before the high-low pressure runs were made, its "fundamental interval" was determined just prior to these runs, and its ice-point just after. No appreciable change was evident.

[25] Tower, "Conductivity of Liquids," Chem. Pub. Co., p. 41 (1905).
[26] Mueller and Sligh, *J. O. S. A.*, **6**, 958 (1922).
[27] Mueller, *Bur. Stan. Sci. Paper*, **339**, 183 (1919).
[28] Int. Crit. Tables, p. 53 (1926).
[29] Ref. 27.
[30] Ref. 24.

TABLE I.

CHARACTERISTICS OF PLATINUM RESISTANCE THERMOMETERS.

Thermometer.	Ice.				Steam.				Sulphur Vapor.		
	Ice Sample.	Conductivity of water from ice.	Date.	R_0.	Date.	R_{100}.	F.I.	$\frac{R_{100}}{R_0}$.	Sample.	Date.	δ.
E.F.I.6.7-1.....	No. 3	3.928×10^{-6}	July 7, 25	16.9477	June 17, 25	23.5275	6.5798	1.3882	No. 1	June 25, 25	1.504_0
	No. 4	4.019×10^{-6}	Aug. 8, "	16.9480	Aug. 8, "	23.5278	6.5798	1.3882			
	No. 4	4.019×10^{-6}	Aug. 8, "	16.9480	Aug. 26, "	23.5279	6.5799	1.3882			
	No. 5	3.148×10^{-6}	Sept. 5, "	16.9481	Sept. 5, "	23.5282	6.5801	1.3882	No. 2	Sept. 3, 25	1.503_8
				Ave. 16.9480		Ave.	6.5799			Ave.	1.503_9
E.F.I.7.2-2.....	No. 3	3.928×10^{-6}	July 7, 25	18.1741	June 17, 25	25.2325	7.0584	1.3884	No. 1	June 14, 25	1.501_7
	No. 4	4.019×10^{-6}	Aug. 8, "	18.1747	Aug. 8, "	25.2332	7.0585	1.3884			
	No. 4	4.019×10^{-6}	Aug. 8, "	18.1747	Aug. 26, "	25.2331	7.0584	1.3884			
	No. 5	3.148×10^{-6}	Sept. 5, "	18.1745	Sept. 5, "	25.2329	7.0584	1.3884	No. 2	Sept. 3, 25	1.499_8
				Ave. 18.1745		Ave.	7.0584			Ave.	1.500_8
E.L.-3.....	No. 3	3.928×10^{-6}	July 7, 25	15.0732	July 7, 25	20.9342	5.8610	1.3888	No. 2	June 17, 25	1.500_6
	No. 4	4.019×10^{-6}	Aug. 8, "	15.0732	Aug. 8, "	20.9342	5.8610	1.3888			
	No. 4	4.019×10^{-6}	Aug. 8, "	15.0732	Aug. 26, "	20.9342	5.8610	1.3888			
	No. 5	3.148×10^{-6}	Sept. 5, "	15.0732	Sept. 5, "	20.9340	5.8608	1.3888	No. 2	Sept. 3, 25	1.501_4
				Ave. 15.0732		Ave.	5.8610			Ave.	1.501_0
	No. 7	4.569×10^{-6}	Aug. 17, 26	15.0736	Aug. 18, 26	20.9344	5.8608	1.3888			
	No. 8	3.575×10^{-6}	Sept. 5, "	15.0736							

TABLE II.

CALCULATION OF NAPHTHALENE BOILING-TEMPERATURES BY FORMULA

$\log t_p = 0.20248 \log p + 1.755102$ ($t_{760} = 217.976°$).

p.	$t_{p(\text{calc.})}$.	$t_{p(\text{obs.})}$.	$t_{p(\text{obs.})} - t_{p(\text{calc.})}$.
824.15	221.582°	221.581°	− .001°
824.01	221.575	221.576	+ .001
823.74	221.560	221.563	+ .003
800.73	220.293	220.295	+ .002
800.73	220.293	220.297	+ .004
800.69	220.290	220.297	+ .007
800.58	220.284	220.293	+ .009
780.81	219.172	219.171	− .001
780.71	219.166	219.164	− .002
780.67	219.164	219.164	± .0
767.78	218.427	218.424	− .003
767.62	218.417	218.421	+ .004
767.56	218.414	218.406	− .008
765.90	218.318	218.312	− .006
765.83	218.314	218.319	+ .005
765.81	218.313	218.309	− .004
765.79	218.312	218.299	− .013
*758.45	217.887	217.892	+ .005
*758.40	217.884	217.884	± .0
*758.32	217.879	217.884	+ .005
*757.50	217.831	217.828	− .003
*757.50	217.831	217.828	− .003
*757.48	217.830	217.826	− .004
748.84	217.324	217.327	+ .003
748.72	217.317	217.322	+ .005
748.37	217.297	217.302	+ .005
748.16	217.285	217.290	+ .005
*742.13	216.929	216.926	− .003
*742.12	216.928	216.932	+ .004
*742.06	216.925	216.923	− .002
732.79	216.373	216.379	+ .006
732.52	216.357	216.359	+ .002
732.04	216.328	216.328	± .0
731.19	216.277	216.278	+ .001
*726.39	215.989	215.987	− .002
*726.37	215.987	215.985	− .002
*725.64	215.944	215.940	− .004
705.81	214.735	214.732	− .003
705.43	214.712	214.716	+ .004
705.08	214.690	214.688	− .002
704.58	214.660	214.658	− .002
704.15	214.633	214.630	− .003

In Table II are given the various reduced pressures at which temperature determinations were made, the observed temperatures corresponding to them, and temperatures calculated from the reduced pressures by means of the formula,

$$\log t_p = 0.20248 \log p + 1.755102.$$

This formula makes 217.97$_6$° the boiling-point of naphthalene under normal pressure.

TABLE III.

t_{760} CALCULATED BY DIFFERENT FORMULÆ.

Holborn and Henning: $t_{760} = t_p - 0.0585(p - 760) + 0.000025(p - 760)^2$.
Finck and Wilhelm: *$t_{760} = t_p - 0.2073(t_p + 273.1) \log (P/760)$.
Eppley: $\log t_p = 0.20248 \log p + 1.755102$.

Data.					$t_{760(calc.)}$.		
Date.	Thermo.	Naphthalene.	p.	$t_{p(obs.)}$.	Holborn and Henning.	Finck and Wilhelm.	Eppley.
Sept. 1925....	E.F.I.6.7-1	S-8	759.84	217.963°	217°.972	217°.972	217°.972
			760.06	217.976	217 .972	217 .972	217 .972
			760.10	217.978	217 .972	217 .972	217 .972
			762.11	218.092	217 .969	217 .969	217 .969
			762.17	218.092	217 .965	217 .966	217 .966
			762.23	218.096	217 .966	217 .966	217 .966
					217 .969$_3$	217 .969$_8$	217 .969$_5$
Sept. 1925....	E.F.I.7.2-2	S-8	762.69	218.133	217 .976	217 .976	217 .976
			762.69	218.134	217 .977	217 .977	217 .977
			762.73	218.136	217 .977	217 .977	217 .977
			762.77	218.139	217 .977	217 .978	217 .978
			762.77	218.140	217 .978	217 .979	217 .979
			762.81	218.142	217 .978	217 .978	217 .978
					217 .977$_2$	217 .977$_8$	217 .977$_8$
Sept. 1925....	E.L.-3	S-8	758.53	217.878	217 .964	217 .963	217 .963
			758.58	217.882	217 .965	217 .964	217 .964
			758.58	217.882	217 .965	217 .964	217 .964
			760.51	218.000	217 .970	217 .969	217 .970
			760.56	218.004	217 .971	217 .971	217 .971
			760.58	218.005	217 .971	217 .972	217 .971
					217 .967$_7$	217 .967$_2$	217 .967$_2$
Aug. 27, 1926.	E.L.-3	S-8	757.50	217.828	217 .974	217 .974	217 .973
			757.50	217.828	217 .974	217 .974	217 .973
			757.48	217.826	217 .974	217 .972	217 .972
Sept. 1, 1926..	E.L.-3	S-9	758.45	217.892	217 .983	217 .982	217 .981
			758.40	217.884	217 .978	217 .977	217 .976
			758.32	217.884	217 .982	217 .982	217 .981
					217 .977$_8$	217 .976$_8$	217 .976$_0$
		Mean of all			217°.973	217°.973	217°.973

B.P. of "B. of S." Naphthalene = 217.97$_3$° ± 0.001.

* Constant A = (0.2073) has been modified according to personal communication from Dr. E. F. Mueller, Aug. 4, 1926.

Readings marked with an asterisk were made under atmospheric pressure. Those marked with a "g" were made about one year before the others, with a water-sealed gasometer to vary the pressure or to maintain it constant.

In Table III are gathered the determinations of the boiling-points of this "Bureau of Standards" naphthalene made under the several atmospheric pressures listed, with three different thermometers. There are given also the boiling-points corresponding to these pressures calculated for normal pressure by the formulas of Holborn and Henning,[31] Finck and Wilhelm,[32] and the logarithmic formula above, respectively. (With the logarithmic formula, the boiling-point for the corrected pressure first was calculated. Then the difference between this value and the value for the boiling-point under normal pressure given by the formula, namely 217.976°, was found. Finally this difference was added to or subtracted from the corresponding observed boiling-point.)

The boiling-point of the sample of naphthalene used, under normal pressure, on the basis of these figures, is $217.97_3° \pm 0.001$.

TABLE IV.

DETERMINATIONS OF NAPHTHALENE BOILING-POINT WITH RESISTANCE THERMOMETER.

Date.	Observer.	Naphth. B.P.	S.B.P.	δ.	Naphth. B.P. $p=760$, S.B.P. $=444.6°$.
1905	Travers and Gwyer [34]	218.04°	444.53°	1.5	218.05°
1910	Waidner and Burgess [35]	217.98	444.70	1.503 (Aver.)	217.96
1911	Holborn and Henning [36]	217.96	444.51	1.490 (Aver.)	217.97
1925	Finck and Wilhelm [37]	217.95	444.6	1.493	217.95
1926	Eppley	217.97	444.6	1.502 (Aver.)	217.97
				Mean	217.98°

[31] Holborn and Henning, *Ann. d. Physik*, **4**, 26, 865 (1908).
[32] Finck and Wilhelm, *J. Am. Chem. Soc.*, **47**, 1577 (1925).
[34] Ref. 4.
[35] Ref. 6.
[36] "Handbuch d. Physik," Vol. IX, p. 597 (J. Springer, Berlin, 1926).
[37] Ref. 12.

It becomes interesting, therefore, to compare this value with those obtained in published determinations with the platinum resistance thermometer. Table IV gives these, one only (by Callendar and Griffiths [33]) being omitted, because of the high δ value of the wire used.

The unweighted mean of these values is 217.98° with which both the value 217.97$_3$°, found by experiment for the boiling-point at normal pressure, and the value 217.97$_6$°, given for normal pressure by the logarithmic formula, are in good agreement.

SUMMARY.

A formula is derived by which a number of boiling-points of "Bureau of Standards" naphthalene between 824 and 704 millimeters of mercury are calculated with a maximum deviation of 0.013°, and an average deviation of 0.004°, from those determined experimentally.

The boiling-points of "Bureau of Standards" naphthalene in the neighborhood of $p = 760$ millimeters of mercury, determined with three platinum-resistance thermometers, are calculated to normal pressure by the formulæ of Holborn and Henning, Finck and Wilhelm, and the author's logarithmic formula above.

The mean of the values in each case is found to be 217.97$_3$°, with a mean error of the mean of $\pm$ .001° in the value calculated by the logarithmic formula.

This figure (217.97$_3$°) is in good agreement with 217.98°, the average of five determinations of the naphthalene boiling-point with the platinum-resistance thermometer—four earlier determinations and the present one.

The value 217.97$_3$° is also in good agreement with that of 217.97$_6$°, given by the logarithmic formula for $p = 760$ millimeters of mercury.

[33] Callendar and Griffiths, *Phil. Trans. Roy. Soc. Lon.*, A, **182**, 119–157 (1891).

ECONOMIC ASPECTS OF ENGINEERING APPLICATIONS OF STATISTICAL METHODS.

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OBJECT.

HERE and there in the engineering literature [1] of recent years we may find an occasional reference to the application of statistical methods to particular engineering problems. Furthermore the necessity for estimating the probable errors of experimentally determined results has been accepted in general by the engineering profession.

This note calls attention to some additional applications of modern mathematical statistical theory, to research, design, production, inspection, supply and other engineering problems. Attention is given to certain general types of problems in the solution of which statistical applications have been made, and to the nature of the possible economies effected thereby. It is reasonable to believe that very definite economic advantages can be obtained in any large industry through such applications.

ECONOMIC CONSIDERATIONS.

In the application of statistics, as in the application of scientific laws in general, it is often very difficult to form even an approximate estimate of the magnitude of the resulting economic advantages and hence no effort will be made to give specific figures.

Some very important problems call for statistical solutions. One general type is the estimation of safety factors to allow for the happening of the unexpected. The failure of a piece of apparatus may involve, in addition to great financial loss, a serious injury or even death to one or more individuals. Telephone engineering illustrations could be mentioned but

[1] See appended list of references.

we shall consider a problem arising outside this field, because it has already been discussed in the literature.

The civil engineer often must build levees or dams to take care of exceptional rainfall. Customarily he must make his estimate of the limiting flood height upon the basis of a few yearly records of the yearly run-off of the given area. Too low an estimate may result in a destructive flood. Of course this gives rise to a difficult problem in the solution of which modern statistical theory may prove to be a helpful tool.[2]

Another general problem is that of setting engineering standards, where, as is often the case, the underlying experimental data may show considerable dispersion. For example, the strength of a given kind of telephone pole is a function of its modulus of rupture. If we use standard methods and measure the modulus of rupture of each of a number of poles which appear to form a homogeneous group, we must expect to find even wide differences between the observed values because there are many factors influencing the modulus of rupture not easily discernible or at least not easily measurable and controllable. Indications are, however, that these causal factors approximate a constant system of chance causes, and hence that probability theory is applicable.

Assume that these differences in observed values of modulus of rupture for the different poles can be sufficiently well characterized by two parameters; viz., the average and the standard deviation. It is of great economic importance to obtain the best possible estimates of the expected values of these two factors as we shall now see.

If the average taken as standard happens to be in excess of the expected modulus, serious losses may be incurred through breaking of telephone poles under storm conditions; on the other hand, if the chosen standard happens to be below the expected value, losses estimated in hundreds of thousands of dollars are incurred through wholesale removal of telephone poles a number of years before such removal is actually

[2] Goodrich, R. W., "Straight Line Plotting of Skew Frequency Data," *Trans. of Amer. Soc. of Civil Eng.*, pp. 50–54, paper No. 1622, 1927.

necessary. Modern statistical methods assist an engineer in forming the best estimate of such standards in terms of the observed data, by making it possible for him to allow for sample size, to correct for error of measurement and to insure a homogeneous sample upon which to base his estimates.

APPLICATIONS TO RESEARCH, DEVELOPMENT AND DESIGN.

The results of research are almost always expressed in terms of averages and errors of averages, but engineering methods of calculating such errors have not kept pace with the rapid development in statistical theory. Particularly in cases of small numbers of measurements, errors calculated by the customary methods are much too small. In fact our best estimates may be 100 per cent. or 200 per cent. higher than those customarily used.

Even on general principles it goes without saying that, if it is worth while making an estimate of an error, it is worth while doing it to the best of our ability. Nothing short of the best estimates can satisfy the modern research engineer, particularly since these do not involve any additional labor. However, in certain classes of cases the economic importance of attaining the best estimate of an error can be easily illustrated. One such is the determination of the significance of observed differences in the measurements of the physical properties of two kinds of materials or in the qualities of operation of two experimental models of a given kind of apparatus. A specific instance would be that of comparing the electrical characteristics of two contact alloys, one the standard now in use and another a much cheaper material. In such instances we have two sets of experimental data corresponding to either the two kinds of material or the two kinds of model. We must determine if the observed differences between the two sets are reasonably attributable to chance. If they are and if we must select one or the other upon the basis of the available data without further experimentation, then we are free to choose the most economical material or design, even though the observed quality of the cheaper material is not quite up to standard.

Another application arises in building up any kind of apparatus out of a large number of piece-parts. To secure the economies of wholesale production, piece-parts are customarily manufactured in large quantities. It is a well-recognized fact, however, that the piece-parts of any particular kind are not identical one with the other and that the quality of operation of any assembled piece of apparatus is a function of the qualities of its piece-parts. To assure that a particular piece of apparatus will function within prescribed limits when it is built up of piece-parts selected at random, it is therefore necessary to control the probability distributions of each of the parts so that the resultant chance variation in the quality of the assembled unit will fall within the prescribed limits with a given degree of assurance.

Modern statistical theory shows the cost of reducing the overall chance fluctuation by modifying the chance fluctuation in a given part. Hence it insures an economic distribution of effort in controlling the chance variations of the various piece-parts entering into the system.

To take another very simple design problem, suppose you are building a rack to support a load consisting of the combined weights of different pieces of apparatus. Assume that past experience is available to estimate the average and the standard deviation in the weights of each of the kinds of apparatus. One very customary method of design in such instances is to allow for the maximum load plus a certain safety factor where the maximum load is taken as a sum of the maximum weights that have ever been observed. Of course the chance of obtaining this maximum load is negligibly small, and there is little engineering justification for designing for such a condition because the assurance attained in this instance is out of all proportion usually to the assurance attained at other points in the system. To take a simple case where the standard deviations in all the weights are equal, the satisfactory maximum load is such that the amount added to allow for the dispersions in the separate weights need be only $1/\sqrt{n}$ times as large as that given by the cus-

tomary method considered here. In other words the method often used gives $\sqrt{n}$ times the necessary addition in strength with its associated cost.

Another very general type of problem often arises when we have an observed frequency distribution of some physical quantity and wish to determine the probability that such a distribution could have arisen as a sample from a homogeneous constant system of causes. Thus in setting the standard modulus of rupture of a given kind of telephone pole already referred to, we should use only homogeneous data to obtain the estimate of this standard. Naturally the Chi Square and other tests for goodness of fit give us a check for homogeneity. Instances are at hand where the Chi Square test gave indications of lack of homogeneity not otherwise detected.

Perhaps one of the most general and at the same time most difficult problems is to estimate the degree of dependence of a property of some material or of a piece of apparatus upon some one of its physical characteristics. For example it is known that the moisture content of telephone poles affects their modulus of rupture. Indeed a very definite relationship can be established between moisture content and modulus of rupture of small sawn pieces of timber. However, to determine such a relationship for poles is difficult because we cannot control all factors other than moisture content. The effects of these uncontrolled factors mask that of moisture content so that the degree of relationship between modulus of rupture and moisture content for telephone poles is a problem involving the theory of correlation. Examples of this character could be multiplied in all fields of engineering work and particularly in the field of telephony.

APPLICATIONS TO PRODUCTION, INSPECTION AND SUPPLY.

Inspection engineering has two objects: to protect the consumer and to effect economies in the method of production. To see how these two objects are attained it is of interest to consider briefly some of the details of the manufacturing problem. The telephone instrument, which is so familiar to

everyone, is not so simple as it looks. To make it requires 201 parts and to connect it to another instrument requires approximately 110,000 more parts. The annual production of most of these parts runs into the millions, so that the total annual production of parts runs into the billions.

Twenty or more raw materials such as gold, platinum, silver, copper, tin, lead, wool, rubber, silk and so forth, literally collected from the four corners of the earth, are used in the manufacturing process to produce this great quantity of piece-parts. The 100 per cent. inspection given all telephone equipment at the time of installation attains the first object—the protection of the consumer. It is, as it were, the consumer's watch-dog. Modern applications of statistical theory to inspection, however, have to do with the second object, the economic phases of which can best be divided into five parts:

1. To determine, for each step in the process inspection, the economic percentage rejection or tolerance for defectives.
2. To reject defective material at such points in the chain of production as will make the net cost of rejection a minimum.
3. To determine for each step the minimum amount of inspection which will suffice to give economic control of quality. In other words, to introduce sampling inspection wherever possible.
4. To detect lack of control of quality or trends and erratic fluctuations.
5. To assist in finding the causes of these trends and to assist in their control wherever necessary.

Some of the statistical problems incidental to carrying on this work are discussed in publications from these Laboratories.[3] Through application of statistical theory to date, a very appreciable percentage reduction in the cost of inspection

[3] "Quality of Telephone Materials," Jones, R. L., *Bell Telephone Quarterly*, Vol. VI, pp. 32–46, January, 1927.

Other references given at the end of this paper.

has been attained. The estimated annual savings to the Bell System accruing from carrying out the third object alone more than justifies all of the attention that has been given to such studies.

In production engineering where there are, in general, large quantities of data to be analyzed, even the choice of the method of analysis to be used may lead to appreciable savings through reduction of the number of observations required. A typical case is that where the estimate of the standard deviation may be obtained by either the root mean square or mean error method. Because of the greater efficiency of the root mean square method, fewer observations are required to obtain a certain degree of precision with this method than with the other one and annual savings effected in this manner may run into the thousands of dollars for a single kind of apparatus.

We shall close with a supply problem. Telephone poles are supplied to the trade in a comparatively large number of classes to meet the various needs. To be specific, let N be the expected number of poles per acre, m be the number of sub-classes of poles defined in the specifications, $p_1'N$, $p_2'N$, $\cdots$, $p_m'N$ the estimated expectancies of poles per sub-class per acre, p_1M, p_2M, $\cdots$, p_mM the estimated expectancies of poles supplied to the trade per sub-class upon the basis of past experience, X_1 be the total expected cost of poles if $p_1'M$, $p_2'M$, $\cdots$, $p_m'M$ of each class are used and let X_2 be the total cost if p_1M, p_2M, $\cdots$, p_mM of each class are used, where M is the total number of poles used per year. Can we effect an appreciable annual saving by taking all possible steps to make $p_i' = p_i$ for $i = 1, 2, \cdots, m$?

Of course the two sets of estimated expectancies are subject to sampling errors and hence the observed differences between the two sets may or may not be attributable to chance. If they are, it would be useless to try to effect a savings in the way just indicated. On the other hand, if some test such as Chi Square indicates a significant difference between these two sets, then an attempt may be made to

secure the economies accruing from making $p_i' = p_i$ for $i = 1, 2, \cdots, m$.

In one such study preliminary results indicate that an annual saving running into thousands of dollars may be made possible by securing a consumption of poles more nearly consistent with the expected frequency distribution into sub-classes as given by nature.

SOME GENERAL REMARKS.

We might continue at great length to formulate particular applications or even classes of applications of statistical theory to engineering problems. It is hoped that enough has been said, however, to give the statistically trained reader a glimpse of possible applications in a comparatively new field and to indicate to business men who may read this note the nature of economies that it should be possible to effect through such applications. Nevertheless we must consider a word of warning and a few generalizations.

There are many engineers who discredit almost entirely any results or conclusions derived through statistical interpretation. They often say, "You can prove anything by statistics." But this comment was well answered recently by the English statistician Yule[4] who said, while commenting on this remark, "'You can prove anything by statistics' is a common gibe. Its contrary is more nearly true—you can never prove anything by statistics." It is probable that much of the criticism of the applicability of statistical methods in various fields may be attributable to misconceptions of the import of modern statistical theory developed at the hands of such men as Laplace, Gram, Charlier, Thiele, Tchuproff, Markoff, Pearson, Edgeworth and others. True it is that the zeal of the applied statistician may at times carry him beyond the barrier of limitations so carefully set up by the above group of men and their followers. Such a

[4] "The Function of Statistical Method in Scientific Investigation," Yule, G. U., published by the Industrial Fatigue Research Board, Report 28, London, 1925.

statistician may derive conclusions which are not consistent with what would even be termed common sense. In this connection the engineer statistician may profit materially by reading two popular addresses by Professor E. B. Wilson,[5] one of which he closes in the following way: "and as the use of the statistical method spreads we must and shall appreciate the fact that it, like other methods, is not a substitute for but a humble aid to the formation of scientific judgment. Only with this philosophy in mind may we truly hope, with care, to avoid, in the main, being classed in the superlative category of that oft-cited sequence of liars, damned liars and statisticians!"

From a statistician's viewpoint, all engineering data may be considered as a sample. The engineer and man of business have for several years been making applications of statistical methods for representing these data. In fact it is this phase of the subject which up to the present time has received practically all the consideration in textbooks on the application of statistics to industry. It will be noted, however, that the problems raised in this paper are of a different nature and have to do with making use of data constituting past experience in guiding future engineering activities. All of these problems call for sampling theory and thus throw open an almost uncharted field of application. It is the interpretation of causal relationships underlying sampling theory that appeals to the engineer who himself believes, as did Thiele, that "Everything that exists and everything that happens is a consequence of a previous state of things." [6]

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Magnesite Industry of India. K. R. NATARAJAN (*Ind. Eng. Chem., News Ed.*, 1928, *6*, no. 2, 8) gives a concise summary of the magnesite industry of British India. The largest of the deposits occur near Salem in south India, are known as chalk hills, and have been worked for over thirty years. The raw ore is calcined in kilns. Caustic magnesite is produced at a temperature of 800° C., dead or burnt magnesia at a temperature of 1500° C. Over ninety per cent. of the product is exported, chiefly to the United Kingdom, United States, and Belgium, also to France, the Netherlands, Germany, Italy, Japan, and other countries. The average mass exported yearly was 607,000 tons prior to the war, and has been 648,000 tons since the war; the average during the war was 474,000 tons. Caustic magnesite and magnesium chloride are used in cement. Burnt magnesite is used as a refractory and in pipe coverings. Magnesia is applied in numerous industries, such as the manufacture of paper, paints, dynamite, toilet preparations, metallic magnesium, magnesium salts, glass, porcelain, and rubber, in wood preservation, and in water purification. J. S. H.

Divining Rod. W. J. LEWIS ABBOTT (*Science Progress*, 1928, *22*, 483–486) describes his experience, covering a period of more than 35 years, with dowsers or diviners, who use a rod, usually of hazel, to locate underground waters as a preliminary to sinking a well. Abbott writes of the diviner:—"I can safely say that I never knew of a single case in which he located water in a place that would have surprised a hydrologist: on the other hand, in almost every case he has been hopelessly wrong, and has often plunged the dupe into useless and great expense." The diviner is guided by surface features, and is ignorant of geology. J. S. H.

New Natural Vanadates. WILLIAM F. FOSHAG and FRANK L. HESS (*Proc. U. S. Nat. Museum*, 1927, *72*, Article 11, 1–12) describe two new natural vanadates of calcium, which they have named rossite and metarossite. They are simple hydrous calcium vanadates, the respective formulae being $CaO.V_2O_5.4H_2O$ for rossite, and $CaO.V_2O_5.2H_2O$ for metarossite. Both are soluble in water; and metarossite is a dehydration product of rossite. The new minerals occur in Bull Pen Canyon, San Miguel County, Colorado, in sandstone which also contains carnotite. J. S. H.

NOTES FROM THE U. S. BUREAU OF STANDARDS.*

INTERNATIONAL COMPARISON OF RADIO FREQUENCY STANDARDS.

THE increase in power of many United States and foreign radio stations, making them international in their effects, has raised the question as to whether or not the national standards of radio frequency of the various governments are in agreement. Since 1924, the bureau has made several comparisons of frequency standards with the national laboratories of England, France, Italy, Germany, Canada, and Japan. These showed satisfactory agreement to the accuracy then required.

During the past year, however, it has become important to know much more accurately the agreement of the standards of the different nations. The development of the temperature-controlled piezo oscillator offered a means of attaining this. Accordingly, during the summer of 1927 the chief of the bureau's radio section, took to Europe such a piezo oscillator containing two quartz plates which were carefully calibrated according to the United States standards. The piezo oscillator was so constructed that the conditions of operation (tube voltages, temperature, etc.) could be very accurately reproduced at any place. Measurements were made on these piezo oscillators at the National Physical Laboratory, England; Military Telegraph Laboratory, France; Italian Naval Laboratory, Italy; and the National Physical & Technical Laboratory, Germany; where the national standards of the respective countries are maintained.

The differences between the measurements made at the various laboratories were very small, the average departures from the mean being 3 parts in 100,000. This agreement is surprisingly good. It represents an average difference of only 0.03 kilocycle at 1000 kilocycles (300 meters). This is much smaller, for instance, than the variation, 0.5 kilocycle, allowed

* Communicated by the Director.

broadcasting stations in this country. In other words, so far as the United States and these four countries are concerned the national standards of frequency agree sufficiently well to insure against interference, provided the transmitting stations are accurately adjusted according to their national standards.

TESTING AND ADJUSTING PIEZO OSCILLATORS.

BY ORDER of the Federal Radio Commission, broadcast stations are required to maintain their frequency within 500 cycles (0.5 kilocycle) of their assigned value. To maintain this accuracy of adjustment it is necessary to have special apparatus for checking the frequency of the transmitting set. The only satisfactory devices at present available for this are piezo oscillators, piezo resonators, and automatic piezo control. A piezo oscillator using a quartz plate is a very satisfactory device and can be purchased commercially. Specifications for a portable piezo oscillator are given in Bureau of Standards Letter Circular 186. The piezo oscillator described in these specifications does not provide for maintaining the quartz plate at constant temperature, which is desirable for the highest accuracy. These specifications do not include directions for cutting and grinding the quartz plate. A suitable plate can be obtained commercially. Letter Circular 223 describes the use of piezo oscillators in radio broadcasting stations. (Copies of these Letter Circulars may be obtained by persons having actual use for them, by addressing the Bureau of Standards, Washington, D. C.)

When a piezo oscillator used as a standard to aid in maintaining the frequency of a station is tested by the Bureau of Standards, there are certain conditions which must be fulfilled. The bureau will undertake a test of a piezo oscillator only upon written request of the owner or operator of the transmitting station in which the piezo oscillator is to be used. This request must contain the following information:

(*a*) Name of the owner of the station where the piezo oscillator is to be used.

(*b*) Location and call letters of the station.
(*c*) Licensed frequency of the station.
(*d*) Type of piezo oscillator and quartz plate used.

There is just at present an exceptional demand for radio tests of this kind which is greatly in excess of the capacity of the bureau for immediate service. For this reason it has been necessary to schedule pending tests and to notify each applicant for test of the approximate date the test will be made. Tests already scheduled will require about two months to complete. Every effort is being made to give much quicker service, consistent with accuracy, after that time.

Assignment of a date for test will be made only upon receipt of the written request from the owner or operator of the station giving the required information. The apparatus may be shipped at the time the test is requested, or later in time to reach the bureau a few days before the assigned date. The test requires not less than two days to complete. It is necessary that the entire piezo oscillator except tubes and batteries be sent to the bureau. The type of tubes and the voltages should be specified in the letter requesting test.

The quartz plate must have a frequency not more than 1 per cent. below the licensed frequency. If it has a frequency higher than the licensed frequency it cannot be adjusted by grinding. The fee for adjustment of quartz plates with mechanical means for adjustment is $12.00. The fee for quartz plates which are not provided with a mechanical means for adjustment and which must therefore be adjusted by grinding is $20. In case it is desired to maintain the quartz plate at a constant controlled temperature higher than room temperature the work involved in the test is much greater. The fees for such tests are $25 and $50, depending on the type of adjustment required.

LARGE DISK OF OPTICAL GLASS COMPLETED.

ON JANUARY 21 the mold containing the large disk of optical glass, cast by the Bureau of Standards on May 7, 1927,

was opened and the glass found to be very good. It appears to be quite uniform throughout and although it contains some seeds and striae, these will not affect its value as a telescope mirror.

The cover was removed in the presence of several distinguished scientists, including Dr. S. W. Stratton, President of Massachusetts Institute of Technology, Mr. W. R. Warner of the firm of Warner and Swasey, internationally known telescope makers, and Dr. George K. Burgess, Director of the Bureau of Standards.

The disk which is about 69 inches in diameter, 11 inches thick and weighs 3500 pounds, will be used as a great concave mirror for the new reflecting telescope of the Perkins Observatory at Ohio Wesleyan University, Delaware, Ohio.

The money with which to establish this Observatory was left to the University by Professor Hiram Mills Perkins of Ohio Wesleyan, who during fifty years of hard work through most rigid economy and sound investment had been able to amass a small fortune, nearly a quarter of a million dollars. It was his desire to establish an observatory of the first rank at the University and that the entire equipment be of American manufacture. The mounting of the telescope was constructed by the famous American telescope makers, Warner and Swasey of Cleveland, Ohio, but difficulty was experienced in getting any bids on the mirror from American glass manufacturers. In particular, no one was willing to state, even approximately, when the disk could be completed. Finally the director of the Observatory, Dr. Clifford C. Crump, called upon the Department of Commerce for assistance. Although the Bureau of Standards has been making optical glass since 1914, no task approaching the magnitude of the present one had ever been attempted.

After four unsuccessful attempts to obtain a disk of the size required, a unique method was developed by the bureau's glass section. 1000 pounds of cullet (broken glass of the same composition as the glass to be made) and 4600 pounds of sand and chemicals were melted in a single large pot in a gas-fired

furnace. The percentage composition of the ingredients used was as follows:

Sand	49.90 per cent.
Boric acid	4.40 " "
Borax	17.55 " "
Soda ash	5.69 " "
Potash	11.97 " "
Barium carbonate	1.86 " "
Saltpeter	6.57 " "
Zinc oxide	1.86 " "
Arsenic	0.30 " "

The molten glass was stirred by hand for six hours and at the proper time on May 7, 1927 the pot was tapped. The glass flowed into a mold of the required size which was specially designed for this purpose by the bureau. The mold was at the same time a carefully insulated annealing furnace, provided with electrical heating elements by means of which the temperature could be adjusted and controlled to within a degree.

The temperature of the glass when poured was about 1350° C. For one week the temperature was slowly lowered until it reached 600° C. The glass was held at this point for about four days to allow the temperature of the glass and furnace to become uniform throughout. At 600° C. this particular kind of glass (borosilicate crown) is quite rigid and yet sufficiently viscous to yield to cooling stresses without danger of cracking.

Beginning on May 18 the glass was allowed to cool slowly at an average rate of $2\frac{1}{2}$° C. per day till 460° C. was reached. It was then annealed at this temperature for six weeks during which time no variation greater than 1° C. was permitted. Final cooling was started on August 30 and room temperature was attained on January 16. The furnace was opened on January 21, and the disk was found to be of excellent quality.

The experience gained for the bureau's scientific staff will be of inestimable advantage, not only to the bureau, but to any American glass makers who may wish to profit by the data obtained, which the bureau hopes to publish in the near future.

ULTRA-VIOLET TRANSMISSION OF GLASSES AND GLASS SUBSTITUTES.

THE number of inquiries which the bureau has received concerning the ultra-violet transmission of glasses and glass substitutes appears to justify the reprinting of the latest (third) edition of Letter Circular No. 235 on this subject in the Technical News Bulletin. The text of this letter circular is as follows:

This letter circular is issued in response to numerous inquiries for information on the transmissive properties of new glasses and organic substitutes for window glass for use in solariums, sun parlors, schools, homes, office buildings, animal houses, and green houses.

The visible rays of light are comprised between the approximate wave-lengths of 760 millimicrons in the red and 400 millimicrons in the violet. Wave-lengths longer than 760 millimicrons are called infra-red, and those shorter than 400 millimicrons, ultra-violet. Though the average eye is not sensitive to wave-lengths shorter than 400 millimicrons such rays actually exist in the light of the sun, extending down to a wave-length a little more or a little less than 300 millimicrons, depending on the time of day, season of year, latitude, altitude and clearness of atmosphere.

Since ordinary window glass shuts out the ultra-violet rays below about 310 millimicrons, much attention has been given of late to the production of special glasses, transparent to the shortest wave-lengths which the atmosphere permits the sun to furnish us. This letter circular gives the results of ultra-violet transmission tests which have been made at the Bureau of Standards upon a number of such special glasses and common window glass.

Total transmission of various glasses for those ultra-violet solar rays to which common window glass is opaque.—Using a filter method, direct measurements, with sunlight as source, have been made during the noon hours of especially clear days from April to December, 1927.

These measurements covered the solar spectral region to

which common window glass is opaque (below about 310 millimicrons). Table 1 gives for that region the total transmission found for the following specimens:

TABLE 1.

Total Transmissions of Various Glasses, when New, for the Ultra-Violet Solar Rays to which Common Window Glass is Opaque.

Trade Name.	Per Cent. Transmission.
Fused quartz	92
Corex	92
Helioglass (Vioray [1])	50
Vitaglass	50
Cel-o-glass [2]	20
Quartz-lite	5
Flexoglass [3]	1
Common Window Glass	0 to 5

Spectral Transmissions of Various Glasses for the Ultra-Violet Rays.—A second and more reliable method of measuring the relative transparency of a specimen of glass to ultra-violet light is to determine its spectral transmission curve, wave-length by wave-length. Curves of this description are shown in the accompanying illustration.[3] These curves were obtained by means of an artificial source of light (quartz mercury arc) giving a line spectrum richer in ultra-violet than the solar spectrum.

By means of these spectral transmission curves an estimate of the relative transmissions of the various specimens for rays shut out by common window glass, may be obtained by reading from the curves the values of the transmission at 302 millimicrons—the wave-length of an intense mercury line, of convenient value for making such tests. Table 2 gives transmission values for this wave-length. These are our most recent findings for new specimens.

[1] Vioray is the foreign trade name for Helioglass.

[2] This consists of a fine wire screen whose interstices are covered with cellulose acetate.

[3] This is a loosely woven fabric usually covered with paraffin.

[4] The chart accompanying the Letter Circular is necessarily omitted.

TABLE 2.

Per Cent. Transmission of Various Glasses at 302 Millimicrons, when new.

Trade Name.	Number of Samples Tested.	Average Thickness.	Average Per Cent. Transmission at 302 Millimicrons.
Fused quartz	1	4.7 mm.	92
Corex	4	2.8	89
Helioglass	15	2.3	56
Vitaglass	15	2.5	44
Cel-o-glass	5	.1 (about)	30
Quartz-lite	16	1.9	0.5
Common Window Glass	14	3.3	0.0

Solarization.—Helioglass, Vitaglass and Cel-o-glass have been found to decrease in transmission at 302 millimicrons and neighboring wave-lengths (295 to 310 millimicrons) by exposure to ultra-violet radiation from the sun, quartz mercury arc, and carbon arc. The rate of change is much more rapid with the arc than with the sun and varies also with the different glasses.

Vitaglass has been on the market the longest, hence concerning this glass we are able to give the most information on solarization. A sample which had been in a hospital window in Rhode Island for a year was found to have a transmission of 25 per cent. at 302 millimicrons (for thickness = 2.3 mm.). Further exposure to the quartz mercury arc reduced the transmission but little, showing that solarization was complete.

Exposure in Washington of a sample (t = 2.35 mm.) of Vitaglass directly to the sun for 123 hours (between 9 A.M. and 3 P.M. during June, July, and August) decreased the transmission from 47 to 35 per cent. Our tests show that the greatest decrease in transmission occurs during the first few weeks' exposure. The average transmission of Vitaglass at 302 millimicrons after complete degeneration by the mercury arc is about 25 per cent. for a thickness of 2.3 mm.

Helioglass has not been on the market for a sufficient length of time to obtain a complete solarization test. Two samples each of Helioglass and Vitaglass, exposed simultaneously to

the sun in October to December, were found to have decreased in transmission about the same rate. The average transmission of Helioglass at 302 millimicrons, after complete degeneration by the mercury arc is about 30 per cent. for a thickness of 2.3 mm.

Corex glass appears to undergo no appreciable change in transmission when exposed to solar radiation. For example, a sample of cathedral finish Corex which had been in a greenhouse roof in New York for 14 months was found to have, as nearly as could be measured on that kind of surface, the same transmission as a new sample. It was then polished plane and found to have a transmission of 89.5 per cent. at 302 millimicrons, while the average transmission for new samples as given in Table 2 is 89.

On the other hand exposure to a quartz mercury arc causes very marked decrease in the transmission.

Cel-o-glass (cellulose acetate) becomes opaque at 302 millimicrons, and shorter wave-lengths, after 25 hours' exposure to the quartz mercury arc. A sample that was exposed to the sun for 400 hours during the months of April to October, decreased but little in transparency at 302 millimicrons. On the other hand, samples that transmitted 30 per cent. at 302 millimicrons, when new, transmitted only 5 to 10 per cent. at this wave-length after being exposed on the side of a building continuously day and night for 8 months, April to December, showing that the change in transparency may be owing to the varying conditions of the weather.

Quartz-lite is not appreciably affected by sunlight. Exposure to the quartz mercury arc decreases its transmission slightly (measured at 313 millimicrons).

Common Window Glass is also slightly decreased in transmission by exposure to the quartz mercury arc but changes inappreciably in sunlight.

Thickness.—The thinner the glass, the greater in general will be its transparency to ultra-violet rays. Considerations of strength, however, set a limit to an indefinite reduction in thickness. Commercial samples have been submitted for test

with a thickness of less than 1 mm. (1/25 inch). In installing such glass, attention should be given to the proper size of sash to meet safety requirements.

THERMAL EXPANSION OF ALLOYS OF THE "STAINLESS IRON" TYPE.

AN INVESTIGATION on the thermal expansion of low-carbon iron chromium alloys of the "stainless iron" type was recently completed. No data on this subject were available. These alloys are being used to an increasing extent for various purposes such as parts of steam turbines, devices for handling acids, and in the manufacture of other articles that must resist corrosion.

A scientific paper which gives results on the expansion of stainless iron for various temperature ranges between room temperature and 1000° C., is now in press. Critical regions were located on some samples of stainless iron. This material expands about 17 per cent. less than pure iron for the temperature range from room temperature to 100° C. The complete report will be valuable to persons interested in this material.

CAST IRON FOR ENAMELING PURPOSES.

THE program of enameling different irons under various conditions, both at the bureau and in the laboratories of two plants, which was mentioned in an item on this subject appearing in the Technical News Bulletin for August, 1927 (No. 124), has been completed. Differences in the blistering tendencies of the different sets of castings are not so apparent in the case of the high lead enamel, but can readily be seen in the case of the higher fired, wet process enamel which was supplied to the bureau for use in this work.

Two irons were used, designated as "L" and "R." Although on the whole there were more blisters obtained with the L than with the R castings, yet the blistering tendencies of the castings were most consistent within the individual heats of either iron. Thus, four heats of each of these irons

were made for this series of tests, and heat 4 of iron L showed distinctly less blistering tendency than the other three heats of the same iron. Similarly heat 1 of the iron R was not as satisfactory as heats 2 and 3 of this iron. These variations may be due to irregularities in the pig iron, melting conditions, or pouring conditions, although the last two conditions were under better control than is commercially practicable.

Some interesting relationships have been found to exist in the blistering tendencies of these various sets of castings. When a surface layer of iron is removed from any of the castings studied—even those most prone to give blisters—the blistering tendency disappears. This surface layer may be removed by machining, pickling under certain conditions, or sufficiently extended sandblasting, with equally beneficial results. Data are being obtained which should indicate whether the improvement can be attributed to bodily removal of blister-producing material in the surface layer of iron, or to some other cause.

A special sandblast apparatus is in use which is capable of delivering a definite amount of sand or other abrasive at a controlled pressure. Preliminary results obtained with this apparatus indicate that those castings which have more tendency to produce blisters under ordinary conditions lose weight less readily under a given sandblast treatment.

PROTECTION OF DURALUMIN AGAINST EMBRITTLEMENT.

THE trend in airplane design toward all-metal construction, and extensive use of the strong light alloy duralumin demands 100 per cent. permanence and reliability of this material. Doubt has been expressed from time to time concerning the permanence of duralumin in service. Sheet duralumin has in a number of cases been seriously impaired in its properties by becoming brittle without any marked accompanying change in its surface appearance. The great bulk of sheet duralumin in use, however, has not shown evidence of such a change and at first manufacturers of materials as well as

builders and users of aircraft held the opinion that no change in the properties of duralumin takes place. However, many well established cases of severe deterioration have been found in sheet duralumin of American and foreign manufacture. These have been shown to be the result of corrosion, the metal being attacked in a peculiar intercrystalline manner.

A coöperative investigation of this subject was conducted by the National Advisory Committee for Aeronautics, the Bureau of Aeronautics of the Navy Department, the Army Air Corps, and the Bureau of Standards. Progress reports are being issued by the National Advisory Committee for Aeronautics in its series of Technical Notes.

Intercrystalline corrosion of sheet duralumin whereby the metal becomes very brittle can be induced by accelerated laboratory tests. Chloride solutions are most effective and the addition of an oxidizer increases the attack very decidedly. By means of such a solution, carried out as an intermittent immersion test on full-size tension bars on which the strength and ductility after corrosion were determined, it has been shown that all of the aluminum alloy sheets of the duralumin type, that is, heat-treatable alloys, are subject, more or less, to intercrystalline corrosion. Heat-treated duralumins of essentially the same composition in the commercial heat-treated condition have been found to differ greatly in their corrosion-resistance. Composition does not appear to be the controlling cause, although alloys containing copper as the main alloying constituent are more susceptible than those containing little or no copper.

It has not been possible by high magnification studies of the structure to obtain definite evidence as to the real "mechanism" of the intercrystalline attack. The real cause evidently is to be attributed to structural changes of a submicroscopic order. Some light has been thrown on this by X-ray analysis, but this method cannot be relied upon as a sure means of distinguishing material susceptible to this form of corrosion from that which is resistant to it.

Cold-working sheet duralumin after heat treatment renders

it somewhat more susceptible to intercrystalline attack. However, this appears to be only a contributory factor. Material which is heat treated by quenching from a temperature considerably below the one at which the alloy constituents are soluble in the aluminum matrix is less resistant to corrosion than if properly heat treated, i.e., quenched from 500° C. and aged at room temperature several days. The rate of quenching, that is, as determined by the quenching medium used, is a very important factor. Sheet quenched in hot water was found to be very much more susceptible than cold-water quenched sheet although the static strength properties and the visible microstructure are the same. Material which, after quenching, is subjected to accelerated aging, that is, aged at some elevated temperature was also found to be much more susceptible to intercrystalline corrosion than material aged at room temperature. For a high degree of corrosion resistance in sheet duralumin (not necessarily complete freedom from attack), the material should be quenched rapidly, as in cold water, from approximately 500–510° C., and aged at room temperature. When exposed to the weather such materials are giving results confirming the conclusions based upon accelerated corrosion tests.

Although the resistance of sheet duralumin to intercrystalline corrosion can be greatly improved by proper heat treatment, protective coatings should also be used in order to assure complete protection. A coating of plain spar varnish is of relatively little use; when pigmented with aluminum powder, however, such a coating is greatly improved and appears entirely suitable to resist most atmospheric attacks. It is not to be relied upon for resistance to sea water, as in pontoons; a heavy bitumastic coating is suitable for this. An oxide coating made by the anodic oxidation process is of little use unless given a supplementary greasing treatment. This greatly improves such a coating and it appears to be very suitable for atmospheric service—not for parts immersed in sea water. The anodic process appears to be the best of the oxide or related processes.

Laboratory tests have indicated that "aluminum-pigmented" rubber coatings are exceptionally well suited for the protection of dynamically stressed parts (that is, parts subjected to vibration or flexure) under very severe corrosive conditions.

Of the possible metal coatings, aluminum appears, from theoretical considerations, to be best. An aluminum coating applied by the metal spraying process gave 100 per cent. protection even under very severe corrosive conditions and after the coated specimens (before corrosion) had been deformed (stretched). A commercial material, "Alclad," the development of which was based upon the information gained from the tests of metal sprayed specimens, has been put on the market by one of the coöperating manufacturers. This seems to be the practical solution of the problem so far as sheet is concerned. In all cases, however, the principles established concerning the proper heat treatment (as explained previously) must be observed if a high resistance to corrosion is to be obtained.

TENDENCY OF MOTOR FUELS TO CAUSE DETONATION.

THE modern motorist is offered a variety of trademarked gasolines by rival filling stations. The major differences between commercial gasolines are of two kinds: (*a*) differences in volatility and (*b*) differences in anti-knock value. The standard distillation test, which is an important part of the Federal motor gasoline specification, enables any oil laboratory to take a cupful of gasoline and determine its relative volatility. There is no similar laboratory test for measuring the relative tendency of gasolines to knock or detonate. All that can be done at present is to compare different gasolines in an actual engine and a survey by the Bureau of Standards shows wide differences between the equipment and methods employed by 20 different laboratories for this purpose. The engines used include regular automobile engines, motorcycle engines, marine engines, farm lighting units, and specially designed single-cylinder research engines. Some of the last-

named are variable compression engines and such an engine may be used to determine the compression ratio at which each fuel starts to knock—this is called the "highest useful compression ratio"—or the compression ratio at which each fuel produces its greatest power, but both these ratings vary with the particular engine used. The methods more generally used in this country depend on finding by trial the amount of tetraethyl lead which must be added to (or the percentage of benzol which must be blended with) the poorer of two fuels in order to give equal knock intensity in a particular high compression engine under specified operating conditions. These methods involve the comparison of all unknown fuels with a standard or reference fuel and the quantity of tetraethyl lead (or benzol) required for a match is termed the tetraethyl lead (or benzol) equivalent of the unknown fuel. Such ratings by different laboratories are not readily compared because (1) each laboratory usually has its own reference fuel and (2) test conditions vary in different laboratories.

The bureau uses a high-compression single-cylinder engine for routine knock testing and adjusts the throttle to prevent excessive detonation with each fuel. The relative power which different fuels develop under these conditions is taken as a measure of their anti-knock value. 15 gasoline samples, submitted by refiners as typical of last winter's production, were found to range from 11 per cent. worse to 30 per cent. better than the U. S. Motor gasoline then being furnished the Government departments under contract. 6 composite gasoline samples, representing the average nonpremium gasoline sold at filling stations in various sections of the country, were also tested. The West Coast sample showed the least tendency to knock, the Texas sample came next and was followed by two East Coast samples. The sample from the Chicago district showed the greatest tendency to knock but calling this 100 the Texas sample was rated 105 and the California sample 118.4.

Atomic Weight of Erbium. ARTHUR EVAN BOSS and B. S. HOPKINS of the University of Illinois (*Journ. Am. Chem. Soc.*, 1928, *50*, 298–300) prepared erbium chloride from gadolinite. They then determined the ratio of erbium chloride to silver in a series of six analyses, and obtained an average value of 167.64 for the atomic weight of erbium. J. S. H.

Synthetic Thyroxin.—Thyroxin is the hormone, or specific secretion, of the thyroid gland. It governs the basal metabolism or basal heat production of the animal body. Harrington and Barger (Biochemical J., 1927, *21*, 169–181) have synthesized thyroxin and determined its constitution. They report that thyroxin is beta-[3 : 5-diiodo-4-(3′ : 5′-diiodo-4′ hydroxyphenoxyl) phenyl]-alpha-aminopropionic acid. J. S. H.

Effect of Salt upon Growing Rice. When salt is present in the irrigation water, rice plants may be injured. G. S. FRAPS (Texas Agricultural Experiment Station Bull. No. 371, 1–10, 1927) finds that, as a conservative limit, pumping should be stopped when the water contains from 40 to 50 grains of sodium chloride per gallon. The extent of the damage depends upon several factors, such as the salt content of the water, the period of time during which it is used, the amount of rainfall, the variety of rice, and the season of the year.
J. S. H.

Production of Indigo Blue by Bacteria. P. H. H. GRAY of the Rothamsted Experiment Station (*Proc. Royal Soc.*, Series B, 1928, *102*, 263–280) has discovered a microorganism, *Pseudomonas indoloxidans*, which occurs in the soil and is able to oxidize indol to indigotin or indigo blue without the production of either indoxyl or isatin. The oxidation occurs in nutrient solutions and on agar media. The indol, however, does not supply the microorganism with energy (food), which must be provided as an organic compound such as tryptophane or, preferably glycerol. Although tryptophane is a precursor of indol, the microorganism does not convert tryptophane into either indol or indigotin. Gray has also discovered that two other microorganisms, *Mycobacterium globeralum* and *Micrococcus piltonensis*, produce small amounts of indigotin when grown on indol agar. J. S. H.

NOTES FROM THE RESEARCH LABORATORY, EASTMAN KODAK COMPANY.*

ANTAGONISM OF RADIATIONS IN PHOTOCHEMICAL AND PHOTOGRAPHIC REACTIONS.[1]

By S. E. Sheppard.

A description of the apparent opposing actions of different radiations on photochemical and photographic reactions.

RECENT ADVANCES IN CELLULOSE AND STARCH CHEMISTRY.[2]

By H. LeB. Gray and C. J. Staud.

The article gives a historical summary of the more fundamental researches concerning the structure of cellulose, beginning with Flechsig in 1883 and concluding with the work of Herzog and of Irvine and Hirst. The results of more recent work include particularly those of investigations conducted during 1926 and 1927. The subject is discussed under the following divisions: Cellulose-structure—Physico-chemical research, Cellulose-structure—Chemical research, Dispersoidology of cellulose, Alkali cellulose, Acids on Cellulose, Viscose, Cellulose esters, Analytical, Biochemical work, and Miscellaneous. The part of the article dealing with starch considers only what appears to be the outstanding work of the past two years. A bibliography of 104 references is given.

* Communicated by the Director.

[1] Communication No. 322 from the Kodak Research Laboratories and published in *Chemical Reviews* 4: 319. 1927.

[2] Communication No. 332 from the Kodak Research Laboratories and published in *Chemical Reviews* 4: 355. 1927.

MOLECULAR ORIENTATION AT SURFACES OF SOLIDS. I MEASUREMENT OF CONTACT ANGLE AND THE WORK OF ADHESION OF ORGANIC SUBSTANCES FOR WATER.[3]

By A. H. Nietz.

A PRELIMINARY study has been made of two principal methods of measuring contact angle. These are (*a*) the Ablett cylinder; (*b*) the plate method. The first of these is considered more or less uncertain at present, since it shows mysterious, unexplainable, and inconsistent results compared with the plate method.

About seventy organic substances have been studied for contact angle, effect on surface tension of water, spreading, and work of adhesion. The results, in all cases, appear to agree well with other results on liquids and solids and with theories proposed by Harkins and Langmuir.

Some evidence on spreading is given. It is shown that some substances which spread violently, only slightly affect the surface tension. Substances which do not spread do not lower the surface tension.

The effect of humidity during crystallization is marked. Increased humidity while solidification takes place causes great increase in work of adhesion. Increase in number of polar groups exposed to water also occurs if the solid is immersed in water.

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[3] Communication No. 323 from the Kodak Research Laboratories and published in *J. Phys. Chem.*, **32**: *255. 1928.*

NOTES FROM THE U. S. BUREAU OF CHEMISTRY AND SOILS.*

NOTES ON THE pH OF CANE JUICE.[1]

By R. T. Balch.

[ABSTRACT]

EXPERIMENTS showed that the application of fertilizers to the soil did not influence the reaction of cane juice. From the results of published investigations on other plant materials, substantiated in part by these experiments, it is concluded that: (1) the pH of the cell sap varies in different parts of the plant, the growing tip having the highest value; (2) the pH of the different parts varies during the development of the plant, and each approaches a fairly constant value as full maturity is reached, namely, at the period of flowering and seeding; (3) temperature and light have a bearing on the pH value of the plant saps; (4) the character of the soil and the fertilizer has only a slight effect on the cell sap of certain parts of the growing plant, and apparently the least effect is upon the sap of the stems, with which the stalk of the cane may be compared.

STUDIES ON GOSSYPOL.

II. CONCERNING THE NATURE OF CARRUTH'S D GOSSYPOL.[2]

By E. P. Clark

[ABSTRACT]

IN the literature D gossypol is considered a derivative of gossypol, which is formed in the cooking and pressing process to which cotton seeds are subjected in the manufacture of cotton seed oil. It forms an aniline compound known as aniline D gossypol. This substance was shown to be identical

* Communicated by the Chief of the Bureau.

[1] Published in *Sugar*, vol. 30, January 1928, pp. 8–9.

[2] Published in *Journal of Biological Chemistry*, vol. 76, no. 1, January 1928, pp. 229–235.

with dianiline gossypol obtained by condensing gossypol with aniline. Upon hydrolysis of aniline D gossypol a substance was obtained which was identical both chemically and physiologically with analytically pure gossypol. These facts render untenable the hypothesis that D gossypol is an oxidation or hydrolytic derivative of gossypol. It has been suggested that in the cooking and pressing process to which cotton seeds are subjected in the manufacture of cotton seed oil the gossypol present in the seeds is bound by condensing with free amino groups of the seed proteins, forming substances *similar in type* to dianiline gossypol.

OPTICAL ROTATION AND ATOMIC DIMENSION. VII. THE HALOGENO-HEPTA-ACETYL DERIVATIVES OF GENTIOBIOSE.[8]

By D. H. Brauns.

[ABSTRACT]

THE α-fluoro-, chloro-, bromo- and iodo-hepta-acetyl derivatives of gentiobiose were prepared and described. A comparison of the specific rotational values of these halogen derivatives shows that these straight chain biose derivatives behave the same as the branched chain biose derivatives investigated in the previous article of this series, as agreement with the regular relationship observed for the corresponding derivatives of the monose sugars is obtained only by excluding the values for the fluoro derivatives.

Besides the pure α-halogen derivatives of gentiobiose, impure β-chloro and iodo compounds were obtained.

The Walden inversion from β-octa-acetylgentiobiose to α-chloro- and iodo-hepta-acetyl gentiobiose by the action of hydrochloric and hydriodic acids proceeds at least in *two* definite steps, β-chloro- or iodo-hepta-acetyl gentiobiose being intermediately formed.

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[8] Published in the *Journal of the American Chemical Society*, 49, December 1927, pp. 3170–3177.

NOTES FROM THE U. S. BUREAU OF MINES.*

HYDRAULIC CLASSIFICATION OF METAL FEEDS.

A NEW type of hydraulic classifier, devised by the United States Bureau of Mines, Department of Commerce, and which is being used with success in the mills of the Coeur d'Alene mining district in Idaho and elsewhere, is described in Technical Paper 403, by A. W. Fahrenwald, just issued by the Bureau. Results obtained with the classifier show that proper classification of sands increases the recovery of valuable minerals by tabling, improves the grade of the concentrate, and increases the capacity of the tables. This investigation of the subject of hydraulic classification was conducted in coöperation with the School of Mines of the University of Idaho.

The primary objects of the work were to increase efficiency in the concentration of ores on tables and to determine whether classification of the ore pulp by hindered settling would be of benefit in connection with tabling. Consequently, the work involved a careful study of the fundamental principles of hydraulic classification, the practical requirements of hindered settling, and the development of a suitable classifier.

Although the fundamental principles of classification had been worked out by others, little had been done in applying these principles to the classification of table feeds, for the practice of classifying table feeds has been largely abandoned in recent years. In the course of the coöperative investigation attention was given to the density of sand and water mixtures in which the mineral particles were agitated by rising water in glass and in transparent celluloid columns and pockets. The information gained by these experiments led to the development of the new type of classifier embodying the various elements found necessary.

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DETECTION OF CARBON MONOXIDE IN MINES.

Directions for the use of the iodine pentoxide detector in determining the amount of carbon monoxide in mine air have been prepared by the Bureau of Mines in order that mine rescue workers may familiarize themselves with the use of this device, which ascertains the amount of this dangerous gas in mine air quickly, simply, and with reasonable accuracy.

Carbon monoxide is often found in mines, particularly during and after mine fires and explosions, state G. S. McCaa and John A. Davis, in Information Circular 6057, recently published by the Bureau. This gas has the property of combining with the haemoglobin or red coloring matter in the blood and thus preventing the normal delivery of necessary oxygen to the various tissues of the body. Because of this, headaches and discomfort frequently result after 2 or 3 hours of breathing an atmosphere containing as little as 0.04 per cent. of carbon monoxide, while exposure to a concentration of 0.3 per cent. usually produces unconsciousness in 20 or 30 minutes—followed by death if the victim is not promptly removed to fresh air. The gas is all the more dangerous because it is colorless, odorless, and tasteless, thus affording no warning of its presence that can be perceived by the unaided human senses.

The various methods commonly used by the Bureau of Mines for determining the presence of carbon monoxide in mine air include: observation of the effect on small animals, particularly canaries and mice; the pyrotannic acid method of determining carbon monoxide in blood samples; volumetric gas analysis by absorption or combustion; and the iodine pentoxide method. The underground use of the pyrotannic acid method is somewhat restricted by its limitation to air containing not over 0.2 per cent. of carbon monoxide. Volumetric gas-analysis methods require technical skill and training on the part of the analyst, as well as apparatus that is either too delicate or too cumbersome to be carried about underground, hence these methods are employed almost exclusively in the laboratory. The iodine pentoxide hoolamite method,

however, permits the use of a carbon monoxide detector which readily and simply determines the amount of carbon monoxide in mine air. As the method is of comparatively recent origin, it is not known as well as might be advantageous to men concerned with fighting mine fires or with rescue and recovery after mine explosions.

The most essential feature of the detector is a small glass tube containing granular pumice stone impregnated with a mixture of iodine pentoxide and fuming sulphuric acid. This mixture is called hoolamite after Hoover and Lamb, who patented the formula. When air containing carbon monoxide is forced through the tube by means of a rubber aspirator bulb and suitable valves, iodine is liberated and the original white of the granules is changed to various shades of bluish green or even violet-brown or black, depending upon the amount of carbon monoxide.

One should recognize that the detector does not indicate a deficiency of oxygen in the atmosphere or the presence of such other gases as carbon dioxide and hydrogen sulphide; nor does it automatically give warning of carbon monoxide as the operator may actually be in an atmosphere containing this gas without using the instrument. Hence, in these respects, the detector is inferior to canaries or mice. However, when in good condition and used properly, the detector affords a reasonably accurate quantitative means of determining, while underground, the percentage of carbon monoxide in the air, which it is impossible to do with small animals. Also, birds or mice should be returned to fresh air to revive them after being overcome by carbon monoxide, whereas use of the detector permits a rescue crew to proceed without delay, and additional tests for carbon monoxide can be made as the work progresses. Furthermore, the instrument is light, compact, and easily carried in the mine, needs little attention, and requires no special ability or skill for operation other than that of distinguishing between different shades of color. All of these advantages make the detector an extremely valuable tool for use by men in mine-rescue and recovery work or by

any others who must breathe air in which carbon monoxide may be found.

FORMATION OF PEAT.

In the course of a study of the formation of peat being conducted by the Bureau of Mines, samples of peat have been collected at various depths in Wisconson peat bogs and are being analyzed to determine the rate of decay of various vegetable constituents such as woody matter and cellulose. The main question to be decided is whether wood (lignin) or cellulose, the principal constituents of plants, decay completely in the process of peat formation. If either decays completely, leaving no solid decomposition products, it can be concluded that the other is the original substance from which coal was formed. It is generally conceded that coal was formed from peat. Since the action of bacteria plays an important part in the decay of vegetable matter, a thorough investigation of this phase is now being made. It has been found that bacteria exist at all depths in the peat bogs but they are probably more or less dormant in the lower levels. It has been found further that where bacteria have become dormant their activity is quickly resumed when supplied with available nitrogen.

THE FRANKLIN INSTITUTE.

MINUTE ADOPTED BY THE BOARD OF MANAGERS AT ITS MEETING HELD WEDNESDAY, FEBRUARY 8, 1928, RELATIVE TO THE DEATH OF DR. WM. C. L. EGLIN, PRESIDENT.

In the death of its President, Dr. William Charles Lawson Eglin, The Franklin Institute of the State of Pennsylvania, has experienced an overpowering loss which is deeply felt by all. Both officially and in his personal relations, Dr. Eglin had won the high esteem of all elements of the membership of the Institute. His death is sincerely mourned.

Dr. Eglin was born in Glasgow, Scotland, in 1870, and was educated at the Andersonian University and the University of Glasgow. He came to Philadelphia in 1889 and devoted himself to his work as an electrical engineer. Because of his distinguished services in this field he received the honorary degree of Doctor of Science from Swarthmore College, from the University of Pennsylvania, and from Pennsylvania Military College.

Dr. Eglin joined The Franklin Institute in 1891. He displayed from the outset a lively interest in its activities and in 1898 became a member of the Committee on Science and the Arts, of which body he remained a useful and stimulating member until his death. In 1908 he was elected a member of the Board of Managers. He was made Vice-President in 1922 and in 1924, upon the retirement of the President, was chosen to that office, which he has held until this time.

Dr. Eglin brought to this new office a decision and forcefulness which proved highly effective in meeting a crisis which had suddenly arisen in the affairs of the Institute. These qualities, combined with a generous sense of justice, enabled him to guide the Board of Managers to a proper solution of a critical problem.

In 1924 The Franklin Institute celebrated the centennial of its organization and the completion of a century of honorable service to science and the State. The commemorative exercises were brilliant and were distinguished by the excellent quality of the scores of papers which were presented, as well as by the presence of a great group of scientists of international standing, who delighted to honor the Institute.

In the conception, planning, preparation and guidance of the celebration, Dr. Eglin showed himself to be a leader of vision, ingenuity and courage. He contributed strikingly to the success of the entire occasion by his vivid imagination, his executive ability and his inclusive hospitality. The Institute began the term of its new president and the second century of its life with an honorable public recognition, and with a brightened outlook for future distinction, based upon labors well done.

Dr. Eglin's term as President has been fruitful of good things for The Franklin Institute. He proposed the Christmas Week Lectures for young people and

assisted in their inauguration and maintenance; as chairman, he lent distinction to the meetings over which he was called upon to preside; at all times he was a wise counselor, a constructive critic, an indomitable supporter of the policies of the Institute and of its executive officers.

The Board of Managers of The Franklin Institute desires to express its grateful appreciation of the high value of Dr. Eglin's service as member and officer, and its sense of great and lasting loss in his death. The members of the Board feel a keen personal bereavement in the removal of their President, for whom they had come to know respect, admiration, and warm affection, as he labored without stint for the welfare of the Institute.

(Proceedings of the Stated Meeting Held Wednesday, February 15, 1928.)

THE regular monthly meeting of The Franklin Institute was called to order at eight-eighteen p.m., by Mr. Henry Howson, Vice-President.

He called upon the Secretary to read an official notice of the death of Dr. William Charles Lawson Eglin, President of the Institute, who died on Tuesday, February seventh. The Secretary stated that a minute concerning the death of Dr. Eglin had been adopted by the Board of Managers. It is printed in full herewith. The Secretary attempted to express the common feeling of all the elements of the membership of the Institute concerning Dr. Eglin.

Mr. W. H. Fulweiler arose and said that in his judgment it was particularly appropriate that an expression should be made formally of the sympathy and sorrow of the membership of the Institute, outside the Board of Managers. He therefore moved that a committee of three be appointed by the Chair, to draw up such a minute for presentation to the Institute at its next meeting. This motion was seconded and unanimously adopted.

The Chairman then called for the regular order of business.

The Secretary stated that the minutes of the January meeting had been published in full in the February number of the JOURNAL of the Institute, and moved that they be approved as printed. This motion was unanimously adopted.

He made the following report concerning membership: New members: Life Resident, one; Resident, eleven; Non-Resident, seven; Student, one; Deaths, seven. He referred particularly to the deaths of Dr. H. A. Lorentz and Professor A. E. Outerbridge, Jr. He stated that biographical notes would be published in the subsequent copy of the JOURNAL of the Institute concerning these two former members.

The Chairman then called upon the Secretary to read the appointments to membership in the Committees of the Institute on the Library, Meetings, and Museum. The make-up of these Committees follows:

Committee on Library

C. W. Bates
Charles E. Bonine
H. J. M. Creighton
W. H. Fulweiler
Clarence A. Hall
Joseph S. Hepburn
George A. Hoadley
Lionel F. Levy
M. M. Price
George F. Stradling

Committee on Meetings

Howard McClenahan, Chairman
James Barnes
George S. Crampton
T. G. Delbridge
Arthur W. Goodspeed
Charles Penrose
George D. Rosengarten
Coleman Sellers 3d
W. C. Wagner
W. Chattin Wetherill

Committee on Museum

Hugo Bilgram
G. H. Benzon Jr.
Arthur L. Church
Theobald F. Clark
George A. Hoadley
John J. L. Houston
Henry Leffmann
Frederic Palmer Jr.
H. H. Quimby
William O. Sawtelle

There was no further business, so, at eight-twenty-nine the Chairman introduced in felicitous terms, the lecturer of the evening, Dr. Herbert E. Ives, of the Bell Telephone Laboratories, Inc., New York City. Dr. Ives spoke delightfully on "Television." He outlined the development of this new phase of scientific work, illustrated with demonstrations the principles underlying "Television," and then showed both still and motion pictures illustrating the various problems involved and the methods devised to solve them in the work of the scientific staff of the Bell Laboratories.

Dr. Ives' talk was described as one of the most lucid and enjoyable ever given in the Institute. The attendance was the largest in recent years. About four hundred persons listened to Dr. Ives' lecture and witnessed the demonstrations. About two hundred others had to be turned away.

The meeting adjourned at nine-fifty-six, with an expression of appreciation and gratitude from the Secretary, on behalf of the Institute, to Dr. Ives for coming to deliver his lecture, and with a rising vote of hearty thanks.

HOWARD MCCLENAHAN,
Secretary.

COMMITTEE ON SCIENCE AND THE ARTS.

(*Abstract of Proceedings of Stated Meeting Held Wednesday, February 1, 1928.*)

HALL OF THE COMMITTEE,
PHILADELPHIA, February 1, 1928.

MR. M. M. PRICE *in the Chair.*

The Chairman announced the reelection by the Board of Managers of eighteen members of the Committee to serve for three years and the election of the following new member to serve for three years:

CAPTAIN HIRAM B. ELY.

The Special Committee on Nomination of a Chairman for the ensuing year reported as its choice Doctor George S. Crampton. Doctor Crampton was unanimously elected and took the Chair.

The following report was presented for final action:

No. 2882: Wide Angle, High Speed Lens.

This lens is a wide-angle doublet lens designed for use in astronomical photography and particularly to cover a wide field with flatness of field and freedom from distortion and flare. It is an anastigmatic lens and is corrected for both chromatic and spherical aberration.

The objective is of the type that includes two spaced components. Each of these may be a compound lens, one being positive and the other negative. Photographs of star clusters taken with this lens show exceptionally good definition over the entire field.

"In consideration of his marked contribution to astronomy in the design of wide-angle astrographic lenses of exceptional speed and definition" the award of

the John Price Wetherill Medal is recommended to Doctor Frank E. Ross, of the Yerkes Observatory, Williams Bay, Wisconsin.

The following reports were presented for first reading:

No. 2880: Work of Mr. Arthur Glasgow.

No. 2888: Pyrex Glass.

No. 2891: Literature.

GEO. A. HOADLEY,
Secretary to Committee.

MEMBERSHIP NOTES.

ELECTIONS TO MEMBERSHIP.

(*Stated Meeting, Board of Managers, February 8, 1928.*)

RESIDENT.

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Mr. C. H. Zehnder, New York City.

Miss Marion Reilly, Philadelphia, Pa.

LIBRARY NOTES

Recent Additions

ACKERMANN, A. S. E.—Scientific Paradoxes and Problems and Their Solutions. 1925.
American Foundrymen's Association.—Transactions. Volume 35. 1927.
American Institute of Chemical Engineers.—Transactions. Volume 19. 1927.
American Society for Testing Materials.—Proceedings of the Thirtieth Annual Meeting, 1927. Volume 27, Parts I and II. No date.
Archimède.—Oeuvres Complètes. Traduites du grec en français avec un introduction et des notes par Paul Ver Eecke. 1921.
FABRY, CHARLES.—Introduction Générale à la Photometrie. 1927.
FLEMING, J. A.—The Propagation of Electric Currents in Telephone and Telegraph Conductors. Fourth edition, revised and extended. No date.
GEIGER, H., AND KARL SCHEEL, Editors.—Handbuch der Physik. Volumes 12 and 13. 1927–1928.
Klimschs Jahrbuch. Band 21, 1928.

MAURER, P.—Machinisme et Automatisme. 1927.

Physics in Industry: Lectures delivered before the Institute of Physics. Volume 5. 1927.

SOSMAN, ROBERT B.—Properties of Silica: and introduction to the properties of substances in the solid non-conducting state. 1927.

Union List of Serials in Libraries of the United States and Canada. 1927.

WORDEN, EDWARD CHAUNCEY.—Chemical Patents Index. Volume I. 1927.

World Almanac and Book of Facts for 1928.

BOOK REVIEWS

ENCYCLOPÉDIE PHOTOMÉTRIQUE. Première section—Généralités. Introduction Générale a la Photométrie, par Charles Fabry, Professeur a la Sorbonne et a l'École Polytechnique, Directeur Général de l'Institut d'Optique Théorique et Appliquée. viii–178 pages, 24 x 16 cm., paper. Paris, Revue d'Optique, 1927. Price, paper, 20 francs, bound, 27 francs.

Under the title "Encyclopédie Photométrique" a series of twenty-seven monographs on as many different topics on modern views and applications of photometry is in course of preparation by the Institut d'Optique of which the present volume is the first to appear. The scope and character of the collection and the features of note of this volume are clearly set forth by Professor Fabry in the foreword. He says:

"Under the name 'photometry' is generally understood the measurement of radiations by the visual impressions which they produce. It is in a much broader sense that this term will be understood in the Encyclopédie of which the present volume is the introduction. Regarding the term 'light' in the general sense of 'radiation' whatever may be the wave-length, by 'photometry' will be understood the measure of the intensity of radiations visible or non-visible, irrespective of their position in the spectrum and without regard to measuring appliance employed. In this *ensemble*, the visual measures naturally occupy an important place, but by no means an exclusive one.

"Photometry with its applications in the sense just indicated is a science of vast extent which is scarcely within the domain of one author. Good fortune has brought promises of collaboration from a large number of physicists qualified in diversified branches of the subject. . . .

"This first volume is designed to give the fundamental notions, the definitions and the units which are constantly employed in measurements of radiation. For the reasons just stated, these are based on energy relationships, the 'visual' definitions occupy a secondary place."

In the nine chapters of the volume fundamental principles are set forth in minute detail and in language of the greatest clarity. A feature which will appeal to many readers is the fullness of explanation of the mathematical deductions and the application of graphs and tabular data in dealing with the laws of radiation. There is indeed a notable absence of the mathematical "pyrotechnics" which are often found in books of this type. The chapter headings are as follows: I Simple radiations and their classification; II Energy of Radiation; III Visual measures. Photometric units; IV Relation between visual data and energy data of a radiation; V Polarization of light; VI Photometric properties of bodies;

VII Laws of radiation; VIII Fundamentals of colorimetry, white light; IX Resumé of definitions. Bibliography.

The first volume of this extensive series augurs well for a real advance in the literature of photometry. LUCIEN E. PICOLET.

SCIENTIFIC PARADOXES AND PROBLEMS AND THEIR SOLUTIONS. By A. S. E. Ackermann, B.Sc. (Eng.). With introduction by C. V. Boys, F.R.S. 131 pages, 12mo. London, The Old Westminster Press, 1925. Price, 5 s.

Etymology would not help one much in interpreting the word "paradox" for both its root words have many meanings. "Para" may mean among other things "beside" and "beyond." Chemists know it well in the latter sense. "Doxa" may be "opinion," "expression" and in the oft-quoted chapter in 1 Corinthians it is rendered "glory" ("For star differs from star in glory").

The Greek philosophers delighted in paradoxes and several of them including Achilles and the tortoise and the paradox of motion have been puzzling modern minds for a long while. In this book, many peculiar problems have been collected, the solutions being given in most cases. A few have been left to the ingenuity of the reader. Many will find amusement; a cynic will be inclined to ask why any one should hunt trouble, but the human mind is restless, and millions have of late been worrying over cross-word puzzles, accumulating at much sacrifice of time, a large stock of useless information. It appears that the subject of the book has been made popular having been broadcast from the London station 2 L O. About a year ago Punch called attention to the letter of a London citizen to The Times, protesting against the smoke nuisance, saying that it is just as bad to throw rubbish into the air as into the street, upon which Punch asked "Has the B. B. C. heard of this?" However, the matter of this book does not deserve flippant dismissal, for the author has given much thought to the subject, made an extensive search, and carefully analysed the features of the problems. The abundant responses from the broadcasting show the popularity of the topic.

HENRY LEFFMANN.

PHYSICS IN INDUSTRY. Lectures before the Institute of Physics. By H. E. Wimperis, O.B.E., M.A., F.R.Ae.S. and F. E. Smith, C.B., C.B.E., D.Sc., F.R.S. Vol. 5, 54 pages, 8vo. Oxford University Press, Humphrey Milford, London, Publisher, 1927. Price, 2 s. 6d.

At first thought it would seem that "physics in industry" would involve nothing unusual, for physics, chemistry and mathematics have been employed therein for many years, but a reading of this volume shows that much interesting and valuable material is presented. The first essay is by the Director of Scientific Research in the Air Ministry. The lecturer dwells on the youthfulness of the science of aviation and deals with the later phases of its development and practical application. The data presented are mostly from the British point of view. The statement that Aeronautics was "launched well in the modern scientific path by the formation some seventeen years ago of the Advisory Committee on Aeronautics" seems hardly fair to the pioneers, especially to Langley, in the study of the conditions under which flying is possible. It was a good deal more than seventeen years ago that Langley began to study scientifically the problems of flight, beginning by studying the physics of air-movement and resistance.

During the war the Germans showed that they had taken to heart the work of Langley and his successors, for the Allies were much at the mercy of their enemies. Over seven million dollars damage was done in London in a single night by German flyers, and no commensurate damage was done to the German works.

The lecture on physics in navigation by Dr. Smith, Director of Scientific Research in the Admiralty, covers a number of topics among which may be noted the new process of deep-sea sounding by the "echo" method. The ingenious system of guiding a ship through a narrow channel by the so-called "leader cable" is described, but while in almost all other cases some authority is quoted, this article has no such information. Was the method invented in America, and if so, is this the reason for lack of acknowledgment? HENRY LEFFMANN.

THE CHEMISTRY OF WATER AND SEWAGE TREATMENT. By Arthur M. Buswell, Chief, Illinois State Water Survey, Professor of Sanitary Chemistry, University of Illinois. American Chemical Society Monograph Series. 362 pages, illustrations, 8vo. New York, The Chemical Catalog Company, Inc., 1928. Price, $7.

Sewage disposal is a problem which is scarcely less important in sanitary engineering than any problem now before applied chemistry. The present book is broader than mere discussion of sewage disposal methods for it includes very much information on the industrial uses of water, in fact this portion of the book is about half its extent. The text goes even further, since a very considerable amount of space is devoted to the present-day chemistry including even a chapter on the structure of atoms and the theory of valency. How far this portion is advisable is a question. Undoubtedly the modern theories of physical chemistry, the principles of colloidal action, the effects of varying hydrion concentration, and other developments of the newer chemistry have direct applications to practical purposes. It is a question, however, whether the chemist working in the applied phases of the science will care to take up these more abstruse problems. It would seem that a hydraulic engineer or one engaged in sanitary operations on the large scale such as the management of the water supply and sewage disposal of a large city or the consulting engineer for industrial plants in which water is used for a great variety of purposes often of very different requirements, will be disposed to deal with the practical material before him and give but little attention to the question as whether, for instance, the water molecule is H_2O or some multiple thereof. We must not decry pure science especially in view of the fact that the vast majority of the community looks upon it with some indifference or even disrespect. It is by the cooperative cultivation of theory and practice that we make our substantial progress but so great has the specialism in these lines been developed in recent years and so extensive has become the field of study that there must be separation of the workers and a certain portion must take up the theoretical investigation and another portion must carry out the practical applications. The utilization of both fields of study will be accomplished mostly by joint action of the two classes.

The book is very thorough in its treatment of the subject. The first portion relating to theoretical questions is well written and though in some places decidedly relying upon the higher mathematics, it will give the sanitary engineer a great deal of information. Methods of water treatment for industrial purposes are

given very fully and the second part of the volume which is devoted to sewage disposal covers the whole field as at present available. Among other things very much information is given upon the subject of the organisms which are especially associated with sewage and with sewage filters. Disposal of sewage is an impending problem especially in America where the excessive use of water is so common and the problem has been of late years still further complicated by the introduction of many new forms of industry which give characters to the sewage that interfere materially with the processes that were formerly applicable. The disposal of industrial wastes is indeed an extensive and difficult problem in itself. From these points of view, the book is timely and valuable.

HENRY LEFFMANN.

LES TURBINES A VAPEUR. Traité a l'usage des ingénieurs, des techniciens et des élèves ingénieurs des écoles d'application. Par Ing. Giuseppe Belluzzo, Professeur ordinaire de construction des moteurs thermiques et hydrauliques au Polytechnicum Royal de Milan. Traduit de l'Italien par Jean Chevrier, Ancien élève de l'École Polytechnique, Licencié es Sciences. Deuxieme edition. Vol. I, Théorie et Calcul des Turbines a Vapeur. xvii–367 pages, 24 x 15 cm., paper. Price, 60 francs. Vol. II, Les Turbines a Vapeur. vii–596 pages, 24 x 15 cm., paper. Price, 80 francs. Paris, Gauthier-Villars et Cie, 1927.

The widespread growth of the central electric-power station, whose lines radiate far and wide through rural districts as well as serve urban centers, has all but completely superseded the small isolated power-plant. The use of the internal-combustion motor is still an exception but the steam-engine is now a rarity. Electrical engineers responded promptly to the demand for high-speed generators adapted to direct connection to steam-turbines, and turbo-electric generator for central station equipment has become established practice. While at the zenith of their development, engine-driven generators attained a capacity of the order of 10,000 horse-power, modern demand and progress have brought about turbine-driven generators of the order of 100,000 horse-power, a capacity which far transcends the possibilities of the older type of unit. The steam turbine consequently has achieved a commanding position, and literature dealing with it has grown apace in recent years. Although stable conditions of type and general design of these engines have come about, the art is still relatively new and treatises or texts relating to it are both expected and desired.

An English translation of this work has recently appeared but since that time, a second edition of the original has been issued. This French translation is from the second edition and is unusually up-to-date in having been revised and amplified by the author, so that it constitutes in effect a third edition. The work is comparable in its scope and comprehensive detail with the well-known work of Stodola. Coming from another school, there are necessarily shades of difference of presentation and mode of approach of typical problems which should prove practically helpful.

The first volume is in two main divisions. The first division is on the thermodynamics of steam and its application to steam turbines. The first three chapters are on the fundamental properties of water vapor, the pv, the entropy and the Mollier diagrams with special reference to steam turbines.

The fourth to ninth chapters deal with the flow of steam in stationary or moving, straight and curved conduits, shocks, reaction and work done in the movement along a conduit and experimental researches on the flow of steam.

The second division which begins with the tenth chapter is on the application of the principles discussed in the first part to the next step toward the deduction of directly applicable data to the construction of the operative machine. The preliminaries are given in the tenth chapter, impulse and reaction, partial gate, efficiencies. The eleventh and twelfth chapters deal with the formulation of the flow of steam through the blades and the determination of coefficients for the losses which accompany the flow. In chapters thirteen to sixteen is discussed the determination of the contour of blades for impulse and reaction systems. Chapter seventeen includes a classification of current types of turbines. Procedure in the calculation of steam turbines, the mechanical efficiency, volumetric efficiency, over-all efficiency and steam consumption are treated in the eighteenth to the twenty-first chapters. In chapters twenty-two and twenty-three are the important matters of partial-gate and the governing of steam turbines.

Proceeding to the second volume for which the first volume has been a very complete preparation, we enter the machine-design phase of the subject. Conforming to the order of the first volume, the third part, in twelve chapters, embraces the general plan of a project as governed by requirements and manufacturing considerations. Then, successively, are taken up the construction of guide and rotor blades, the calculation of rotor-drums and discs, the design of rotor-shafts, and finally the structural details of these parts and the method of effecting the static and dynamic balance of the rotor assembly. This matter occupies six chapters.

The construction of the stator-frame is next considered with the allied parts, packing, supporting and thrust bearings and analyzed in two chapters. Throttle valves and governors are given the next chapter. The two final chapters of this division contain a discussion of the structural features, first of high-power turbines, then of moderate power and small turbines.

The features of steam turbines which apply exclusively to marine propulsion are set forth in the fourth part which includes chapters thirteen to eighteen. Here we have a general survey of the general arrangement of the power-plant according to the number of propellers to be driven and the mode of coupling employed. This is followed by chapters on directly coupled turbines and on turbines which drive through reduction-gears. The three final chapters of this division are on methods of best providing for cruising speeds for naval vessels, reversing arrangements and the utilization of the exhaust from auxiliaries. The fifth part of one chapter relates to the application of the turbine to railway engines with discussion and description of an experimental design which was built on plans of the author.

The above summary gives only a much abbreviated outline of the topics treated; they are multiplied many-fold by subdivision. The discussions are all given, with no apparent exception, at ample length and the mathematical analyses, with many illustrative examples, most clearly presented, often with an explanatory note when unusual difficulties arise. The illustrations are both ample in number and satisfactory in quality. Vol. I has 260 figures in the text and 2 plates; Vol. II has 490 figures in the text and 16 plates.

Professor Belluzzo's work is one of those logical and orderly presentations of modern practices in steam-turbine construction that will particularly appeal to readers who desire a practical as well as a complete exposition of the subject.

LUCIEN E. PICOLET.

THE EARTH UPSETS. By Chase Salmon Osborn, B.S., LL.D. 216 pages, 12mo. Baltimore, Waverly Press, Ind., 1927. Price, $3.

It is difficult to appraise this book. It passes from grave to gay, from lively to severe frequently. The author is evidently very well read in earth-lore and familiar with the modern theories as to the structure of our globe. The burden of his story is that the outer shell of the earth which is generally called the lithosphere is not a stable form. There is indeed no doubt of this statement. Within the scope of recorded history we have numerous examples of how this crust slips and breaks and the study of the crust shows that in the past it has suffered very violent changes. Allusions to the earth as solid are more poetical than practical and there is no telling when even those portions of the earth like the eastern coast of North America that show great stability and seem to have shown it through the ages, may indulge in a severe shake. We have had a few of these within recent times that remind us what might happen if a more serious slip or break should occur. The book is divided into short chapters in which various phases of earth motion and instability are considered and the moon comes in for a special chapter. The author seems to lean to the theory that the moon is a mass thrown off from the earth. If so, one would think that it would be a very irregular mass whereas the moon is nearly a perfect sphere so far as we can discern. Of course, it may have been thrown off in a soft condition and by constant rotation assumed its present shape, but the peculiar appearance in the moon's surface with its numerous rings of mountains and sugar-loaf projections in the interior, seems to be a difficulty in this theory. However, a discussion of that subject must be left to the experts in physical astronomy. One of the interesting chapters, too brief really to satisfy many readers, is that on the old theory of the existence of a continent between Europe and America, the lost Atlantis. The author refers to Donnelly's book on the subject which was very well written and attracted a good deal of attention in its day though it is probably now discarded. Some of the text will not be very pleasing reading since references are made to danger spots especially noticeable in certain parts of the world. Italy for instance is noted as having a liability to fracture, but then of course Italians know this from their own history. San Francisco and many other places along the western portion of the continent have been seriously disturbed within comparatively recent periods. Altogether the book is quite interesting and while the author is rather too enthusiastic in some ways, he certainly has, as noted, studied the field in many aspects and produced a volume that is a real contribution to geophysics.

HENRY LEFFMANN.

ATLAS METALLOGRAPHICUS. By Heinrich Hanemann and Angelica Schrader of the Technical Highschool, Berlin. Parts one and two, illustrations, plates, quarto. Berlin, Bornträger Brothers, 1927. Price, 14 marks, 25 pfg. Subscription must be placed for the entire set.

The aim of the authors in the preparation of an "Atlas" to which one can turn for guidance in all things metallographic is a commendable one and their

long and varied experience at the Technische Hochschule at Berlin has given them a great fund of information upon which to draw in the preparation of such a work. Naturally such an Atlas will differ in its treatment of the subject of metallography from the more modest reference books on this subject and will be distinguished primarily by its comprehensive treatment and, as judged by the first two parts, by the very excellent character of the micrographs given. The plan calls for an Atlas in two volumes, the first to be devoted to steel, iron and iron alloys, including coatings, and the second to the non-ferrous metals and alloys, making in all, ten chapters. Each chapter is prefaced by a concise description of the various metallographic constituents together with equilibrium diagrams and a table of suitable etching reagents.

The two parts which have already appeared are devoted to ferrite and plain carbon steels and begin, in the orthodox fashion, with a description of ferrite and show how the structure changes as carbon is added. For this discussion of the carbon steels alone in the annealed condition, 121 micrographs are used each of which, in most cases, receives individual comment to show what the characteristic structural features are. Literally thousands of micrographs will be required for the complete work if it is carried out on the scale on which it has been started.

The loose-leaf form for the plates of micrographs and the accompanying descriptive tables should add materially to the usefulness of the Atlas since this will facilitate its use in the laboratory, at the microscope, instead of limiting it to the library. On the whole, the "Atlas Metallographicus" promises to be a truly monumental work and to serve the students of metallography in much the same way that corresponding works in botany, crystallography and other sciences do.

H. S. RAWDON.

THE STRUCTURE OF THE ATOM. By E. N. da C. Andrade, D.Sc. (London), Ph.D. (Heidelberg), Fellow of University College, London, and Professor of Physics in the Artillery College, Woolwich. Third edition, revised and enlarged. xviii–750 pages, 22 × 14 cm., cloth. New York, Harcourt, Brace and Company, Inc.

If an impulse akin to that which inspired Charles Wagner to write "The Simple Life" something over a quarter of a century ago has not come to the teacher of physical science of recent times, it is not because modern complexities of the fundamentals of physical science have not provided the cause. The striking results of modern investigation in atomic physics have brought about a new atom which, for a full presentation, call for analytical power and scientific inspiration of the calibre of a Newton or a Laplace.

The literature of atomic structure and collateral subjects has grown to vast proportions, some of it immediately applicable in particular fields, but all of it contributing towards establishing atomic theory on a sound basis in harmony with observed phenomena. Out of this accumulated information, Professor Andrade has gathered the material for a comprehensive survey of all that has been done of importance which relates to atomic structure. He says: "A great deal of selection has been necessary to keep the book within the limits of size judged advisable in my choice. I have been guided by the wish to give a coherent and readable exposition rather than a series of abstracts of original papers. Some admirable pieces of work have been deliberately omitted as not falling into the scheme of the book. . . ."

The first edition included matter published up to March 1923. The present third edition has double the number of pages of the former and has been completely rewritten. The character of the work is already known to many, but it may be mentioned for the benefit of readers who may be seeking only a general descriptive text, that the work is an analytical and critical account of some of the most erudite contributions to physical science of the day, and premises on the part of the reader a familiarity with those phenomena of atomic physics which are correlated to the development of the theory of atomic structure.

The work is in two general divisions, Part I, The Nucleus, Part II, The Extranuclear Structure. The additions in the present edition occur mainly in Part II, only relatively small changes having been made in Part I. The chapter headings are: under Part I, I Historical and general, II The passage of swift corpuscles through atoms, III Radioactive considerations, IV The disruption of the nucleus by alpha-particles, V Positive Rays, VI Classical X-ray considerations, VII The structure of the nucleus, VIII A digression on optical spectra. Under Part II, IX The dynamic model of an atom with one electron, X General principles for systems not simply periodic. Hydrogen-like atoms with two quantum numbers, XI The optical spectra of the general atom, XII Excitation potentials and absorption, XIII X-ray spectra and the inner electron groups, XIV The periodic system and the theory of quantum orbits, XV Multiplet structure and anomalous Zeeman effect. XVI Magnetic properties and atomic structure, XVII Static atom models, XVIII Wave theory and quantum theory, XIX Concluding remarks. Numerous bibliographical references relating to the topic discussed are given at the end of each chapter.

The value of a book which has passed through three editions in four years is established. The intrinsic scientific value, the thoroughness of presentation and the straight-forward method employed of indicating inadequacies as well as merits of prevailing theories, contribute to place the work on a high plane among the physicist's sources of information on the most fundamental problem of physics.

L. E. P.

PHYSICO-CHEMICAL METHODS. By Joseph Reilly, M.A. (Cantab.), D.Sc., Dès.Sc., F.Inst.P., F.R.C.Sc.I., M.R.I.A., F.I.C., Professor of Chemistry, National University of Ireland (University College, Cork), William Norman Rae, M.A. (Cantab.), F.I.C., Professor of Chemistry, University College, Colombo, and Thomas Sherlock Wheeler, Ph.D. (London), B.Sc., F.R.C.Sc.I., F.I.C., Chemist, Research Department, Royal Arsenal, Woolwich. xi–735 pages, 21 × 11 cm., cloth. New York, D. Van Nostrand Company. Price, $8.

It is a common enough experience that a long and trying search must often be made to find a desired description and theory of some physical or chemical laboratory apparatus of established value. The authors, having been impressed by the numerous instances in which that difficulty presented itself, have prepared the present volume to bridge this gap in scientific reference literature. In this text-book they have gathered the descriptions of a wide variety of physico-chemical apparatus, with their theories, and methods of approved value which the laboratory worker might only duplicate from other sources by laborious search.

Besides the well-executed diagrams and the full and understandable descriptions of apparatus, their summarized theory, and their manipulation, the

methods of dealing in an orderly and efficient manner with the arithmetical processes which are employed have been given prominence. The work is in ten sections, each of which deals with related topics. The first section deals with the adjustment of observations, nomography (sometimes called "parallel coördinates"), the slide-rule and triangular diagrams. Section two is on the equipment of the laboratory in which are included descriptions of the several well-known types of high-vacuum pumps. Section three, on general operations, includes the cleaning of mercury, filtration, determination of the melting-point, distillation and photography. In section four is covered physical measurements, length, mass, recording of time, temperature and other measurements. Section five is on the properties of solution. Section six considers the properties of gases and vapors. Section seven is on calorimetry. Section eight on optical measurements, covers polarimetry, refractive index, the microscope, colorimetry and nephelometry, photometry and photo-electric effect. Under the caption of electrical properties, of section nine, are included the conductivity of electrolytes, electromotive-force, buffer solutions, hydrogen-ion, electro-chemistry. Section ten is on rates of reaction, X-ray analysis, practical methods in colloid chemistry. An appendix includes tables of elements and their atomic weights, atomic numbers and isotopic weights, and tables of useful constants.

No one can exhaust a subject of this kind but, insofar as it relates to laboratory practice, the subject is covered with adequate fulness and in excellent form.

LUCIEN E. PICOLET.

PUBLICATIONS RECEIVED

The Rise of Modern Physics. A popular sketch by Henry Crew, Ph.D. 356 pages, illustrations, portraits, 12mo. Baltimore, The Williams and Wilkins Company, 1928. Price, $5.

Protective Metallic Coatings, by Henry S. Rawdon. 277 pages, illustrations, 8vo. New York, The Chemical Catalog Company, Inc., 1928. Price, $5.50.

The Mechanism of Homogeneous Organic Reactions from the Physical-Chemical Standpoint, by Francis Owen Rice. New York, The Chemical Catalog Company, Inc., 1928. Price, $5.

The Chemistry of Water and Sewage Treatment, by Arthur M. Buswell. 362 pages, illustrations, 8vo. New York, The Chemical Catalog Company, Inc., 1928. Price, $7.

The Earth Upsets (another terrestrial motion), by Chase Salmon Osborn. 216 pages, 12mo. Baltimore, The Waverly Press, Inc., 1927. Price, $3.

Electric Control Gear and Industrial Electrification, by William Wilson. 361 pages, illustrations, 8vo. London and New York, Oxford University Press, 1927. Price, $8.50.

U. S. Bureau of Mines, Mineral Resources of the United States, 1924, by Frank J. Katz, Chief Engineer, Division of Mineral Resources and Statistics. Part I—Metals, Part II—Nonmetals. 2 vols., tables, plates, 8vo. Washington, Government Printing Office, 1927. Price, $2.

Thermionic Phenomena, by Eugène Bloch, translated by J. R. Clarke. 145 pages, illustrations, 16mo. New York, E. P. Dutton and Company. Price, $2.50.

Wireless Direction Finding and Directional Reception, by R. Keen. Second and enlarged edition, 490 pages, illustrations, 8vo. London, Iliffe and Sons, Ltd., 1927. Price, 21 shillings.

Report of a Survey of Transportation on the State Highway System of Ohio, by The Bureau of Public Roads, U. S. Department of Agriculture and the Ohio Department of Highways and Public Works. 155 pages, illustrations, maps, quarto. 1927.

Publications of the Yerkes Observatory, Vol. V, part 11, Definitive orbit of Comet Delavan 1913f = 1914V, by George Van Biesbroeck. 36 pages, quarto. Chicago, The University of Chicago Press, 1927. Price, $1.50.

Carnegie Institution of Washington. Researches of the Department of Terrestrial Magnetism, volume VI, Land Magnetic and Electric Observations, 1918–1926. Magnetic results, 1921–1926, by H. W. Fisk. Magnetic, atmospheric-electrical, and auroral results, Maud expedition, 1918–1925, by H. U. Sverdrup. 524 pages, illustrations, plates, quarto. Washington, Carnegie Institution, 1927.

Navegação Radiogoniométrica Curvas e Rectas do Azimute. A. Fontoura da Costa. 56 pages, illustrations, 8vo. Lisboa, Imprensa da Armada, 1927.

National Advisory Committee for Aeronautics. Technical Notes. No. 271, Full scale drag tests of various parts of Sperry messenger airplane, by Fred. E. Weick. 6 pages, photographs, quarto. No. 272, Special propeller protractor, by A. L. Heim. 6 pages, photographs, quarto. No. 276, Helium tables, by Lt. Com. Clinton H. Havill, U. S. N. 19 pages, quarto. Washington, Committee, 1928.

Rensselaer Polytechnic Institute. A study of the physical properties of electrolytic cobalt and its alloys with iron, by William Carleton Ellis, 57 pages, illustrations, 8vo. An optical method of determining internal stresses in homogeneous materials, by Charles F. Ffolliott. The measurement of intensive magnetic fields by means of the Zeeman effect on the zinc triplet by Walter C. Michels. 9 pages, illustrations, 8vo. Troy, N. Y., Institute, 1927.

CURRENT TOPICS.

Acid-Forming Properties of Osmium Tetroxide. DON M. YOST and ROBERT J. WHITE, of the California Institute of Technology (*Jour. Am. Chem. Soc.*, 1928, *50*, 81–84), find that osmium tetroxide, OsO_4, is converted into a salt by an aqueous solution of sodium hydroxide, in which the concentration of that salt ranges between 0.0277 and 0.0587 normal. This salt is a derivative of perperosmic acid, H_2OsO_5. Previous investigators have expressed a difference of opinion as to the acid-forming properties of this oxide. J. S. H.

Hydrolysis of Esters in Weak Acid.—Neutral Salt Solutions.—JOHN ERSKINE HAWKINS (Thesis, University of Pennsylvania, 1927, 23 pages) has studied the influence of neutral salts in varying concentrations upon the velocity of the hydrolysis of certain esters by certain organic acids. The reactions studied were those between (1) ethyl acetate, dichloracetic acid, and water; and (2) ethyl formate, monochloracetic acid, and water. The velocity of hydrolysis of esters, catalyzed by a uni-univalent organic acid in the presence of neutral salts is chiefly a function of the hydrogen-ion concentration. J. S. H.

Chemistry of Cotton Seed. M. T. HARRINGTON (Texas Agricultural Experiment Station, Bull. No. 374, 1–19, 1928) has analyzed the meats, hulls, and oil from the seeds of 73 varieties and strains of cotton. The per cent. of oil in the seeds was independent of their size and weight, but usually attained its highest values in those with the highest meat content. Varieties of cotton with lint from $\frac{7}{8}$ to $1\frac{1}{16}$ inches in length had a slightly higher per cent. of oil in their seeds than did varieties with either shorter or longer staple. The oil was determined by extraction with petroleum ether. No appreciable differences were found in certain fat constants (iodine number, saponification number, and index of refraction) of the oil from the seeds of different varieties. Free fatty acids were not present in any of the samples of extracted oil. J. S. H.

The Thickness of Spontaneously Deposited Photoelectrically Active Rubidium Films, Measured Optically. HERBERT E. IVES and A. L. JOHNSRUD. (*Jour. Optical Soc. Am.*, Dec., 1927.) Films of the alkali metals deposited spontaneously in high vacua upon polished platinum or glass are interesting in their behavior. While they are too tenuous to be visible yet they are as effective as considerable quantities of the same metals in displaying photoelectric properties. The purpose of this paper is to determine the thickness of such films. "Plane polarized light polarized at an azimuth of 45° was directed upon the metal or glass surface and the reflected light was examined with a Babinet compensator in conjunction with a nicol prism in order to determine whether the azimuth of the reflected polarized light or its phase were shifted by the presence of the alkali metal film."

Observations were first made upon platinum or glass surfaces free from metallic deposit and were continued for several hours while films were forming upon the surfaces observed. The evidence for the presence of the films was their photoelectric action. When sodium and rubidium were deposited upon platinum no optical change was detected. With rubidium deposited on glass, however, there was found a "definite and measurable change in the phase shift upon reflection accompanied by a very small but definite change in the azimuth of the plane of polarization." In order to correlate these changes with the thickness of the film on glass recourse was had to the electromagnetic theory of the case developed by T. C. Fry. According to this the thickness of the layer of rubidium is about 6×10^{-8} cm. "A probable figure for the effective diameter of a rubidium atom in a crystal lattice is 4.5×10^{-8}. It thus appears from these measurements and computations that the layer of rubidium in question is of the order of magnitude of one atom thick." This estimate comes from the observed change of phase upon reflection. The authors call attention to the possibility that the theory used may not be applicable to an exceedingly thin film. "There is consequently no guarantee that the experimental points do not actually correspond to a thickness of say five or even ten atomic diameters. The real point at issue is however not seriously affected: a film so thin as to be detectable only by one of the most sensitive of optical tests is responsible for the complete photoelectric emission."

G. F. S.